GREATER ANATOLIA AND THE INDO-HITTITE LANGUAGE FAMILY

PAPERS PRESENTED AT A COLLOQUIUM HOSTED BY THE UNIVERSITY OF RICHMOND, MARCH 18–19, 2000

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Robert Drews

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INTRODUCTION AND ACKNOWLEDGMENTS

This book began when Stuart Wheeler, chair of the Department of Classical Studies at the University of Richmond, persuaded his department and university to offer me a visiting appointment for the spring semester of the year 2000. Along with the position came funds sufficient for bringing to the Richmond campus a number of outside speakers. Stuart and I quickly concluded that a series of lecturers, each of whom was likely to address a progressively diminishing audience of undergraduates, was unlikely to do much good for anyone. Better by far would be to spend the money on transportation and accommodations that would bring together for a colloquium eight or ten scholars whose various fields of expertise might illuminate a dark and important topic. In talking to one another, the scholars could demonstrate for serious undergraduates what scholarship was, and how stimulating the life of the mind can be.

An especially promising and timely theme for a colloquium, we decided, would be the relationship of "Greater Anatolia"—the entire land mass between the Aegean and the Caspian—to the origins of Proto-Indo-European. In 1985 Thomas Gamkrelidze and Vyacheslav Ivanov had proposed that Proto-Indo-European was spoken in the valleys of the middle Kura and Araxes, west of the Caspian depression. And two years later Colin Renfrew developed at length a thesis he had first proposed in 1973: that the Indo-European languages originated in one of the cradles of agriculture, the Konya plain of southern Anatolia, and that as a farming population expanded from Anatolia into Europe early in the Neolithic period the farmers brought their Proto-Indo-European language with them. As I had urged with little success in a 1998 *JIES* article, both of these radical views seemed to have not only major flaws but also remarkable promise for solving some hitherto intractable problems about the origins of Proto-Indo-European. Especially did that seem to be the case if the Indo-Hittite theory were thrown into the mix. Neither Gamkrelidze and Ivanov nor Renfrew had fitted their original proposals to the Indo-Hittite theory, which through most of the 1980s was generally considered beyond the pale.[1] Nor had the Indo-Hittite theory been deployed by Andrew and Susan Sherratt, James Mallory, Onofrio Carruba, Marek and Kamil Zvelebil, and other perceptive critics of the proposals that Renfrew, Gamkrelidze, and Ivanov had made—hence our decision to organize a colloquium on the theme, "Greater Anatolia and the Indo-Hittite Language Family."

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[1] The Indo-Hittite theory was sketched out broadly and briefly by Emil Forrer in 1921 but was elaborated by Edgar H. Sturtevant (1875–1952). Sturtevant was instrumental in importing Hittite

To our delight, Renfrew and Ivanov both agreed to participate in the colloquium. In all, eight scholars were invited to make presentations, and two more were asked to offer critical responses. Obviously many more than ten scholars could have made important contributions to the topic, but schedules and resources limited us to a two-day program. Although neither of the two critical respondents was known to be an advocate of either the Indo-Hittite theory or the possibility that our language family originated anywhere in Greater Anatolia, it seemed to us that the colloquium would be most productive if all the scholars making the original presentations had expressed some sympathy for the one theory or the other.

On the linguistic side, in addition to Ivanov, we were fortunate to receive commitments from Bill Darden, Margalit Finkelberg, and Alexander Lehrman. Darden had been at work for some time on criteria for dating the division between Proto-Anatolian and Proto-Indo-European. Finkelberg had in various articles explored the pre-Greek language or languages of the Aegean and the possible relationship of the Linear A language to Indo-Hittite. Lehrman's article, "Indo-Hittite Revisited," in the 1996 volume of *Indogermanische Forschungen*, had ably and vigorously advocated the Indo-Hittite theory. On the archaeological side, Renfrew was joined by Elizabeth Barber, whose work with textiles and most recently with mummies from western China had ramifications both for the Indo-Hittite theory and for the question of the *Urheimat* of the people who "colonized" the Ürümchi oasis. In December of 1998, as planning for the colloquium was already under way, Ryan and Pitman's book on the "Black Sea Flood" was published, and that spurred our invitation to Peter Kuniholm, whose dendrochronological laboratory at Cornell University will eventually help to date the deluge. Finally, Paul Zimansky's outstanding explication

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studies—which were primarily a domain of German-speaking scholars—to the U.S.A. A classical Indo-Europeanist by training, Sturtevant was deeply impressed by Hrozný and Forrer's demonstration that Hittite was "an Indo-European language" and in his middle age began the study of cuneiform and of the Hittite language. In 1931 Sturtevant published his *Hittite Glossary: Words of Known or Conjectured Meaning*. Soon thereafter he helped to secure a Yale appointment for Albrecht Goetze, a Hittitologist in flight from Nazi Germany. With help from Goetze, Sturtevant published a second edition of his glossary in 1936 and a supplement in 1939. By that time Sturtevant had become convinced that Proto-Anatolian, from which Hittite and Luwian evolved, was descended from a language *earlier than and ancestral to* classical PIE. Sturtevant promoted his "Indo-Hittite" hypothesis in lectures, one of which was given at the Linguistic Institute in Ann Arbor, Michigan, in 1938. This lecture was privately printed and narrowly circulated in the U.S., but remained unknown in Europe during the Second World War and its aftermath. The lecture was published posthumously in 1962: see E. H. Sturtevant, "The Indo-Hittite Hypothesis," *Language* 38 (1962): 105–10.

of both the material and the cultural aspects of the kingdom of Urartu put him in a position to shed some badly needed light on the easternmost reaches of Greater Anatolia, an area not documented by Bronze Age tablets.

The specialties represented by our speakers were so disparate that no single sane scholar was likely to accept the role of critical respondent. By parceling the presentations into two series, however, the one dealing mostly with archaeology and the other with language, we were able to persuade two scholars of international renown to play the critical roles. Jeremy Rutter is well known for his meticulous archaeological work on the Neolithic and Bronze Age Aegean, and Craig Melchert for his contributions to Indo-European linguistics and his mastery of the Anatolian languages.

Our "Greater Anatolia and the Indo-Hittite Language Family" colloquium was accordingly held at Richmond, Virginia, on March 17–19 of 2000, under the auspices of the University of Richmond's Department of Classical Studies.The event began at the Virginia Museum of Fine Arts on Friday, March 17, with a public lecture by Lord Renfrew. The colloquium proper began on Saturday, at the University of Richmond campus, and ended with a Sunday session at the Omni Hotel in downtown Richmond. Fifty-nine registrants attended the colloquium, which began with the archaeological presentations and ended with the language papers. Two discussion sessions were scheduled, the first after Rutter's critique and the second after Melchert's. Because the Saturday morning session ended ahead of schedule, an impromptu discussion broke out at that time. Thanks to the technical wizardry of Kenny Morrell (Rhodes Univ.) and Walt Stevenson (Univ. of Richmond), all of the proceedings were "streamed" through the Internet. The archived broadcast remains accessible at the colloquium's web site <http://www.richmond.edu/academics/a&s/classics/Anatolia/index.html>, and listeners can hear for themselves the liveliness of the exchange of ideas and arguments. Excitement was in the air, and the debate—although collegial—was often spirited.

As everyone who has organized a conference will appreciate, its success depends on many people. I thank first of all Stuart Wheeler and Stella Jones, respectively chair and secretary of the Department of Classical Studies at Richmond. Many of the good ideas about the colloquium were Stuart's, and Stella made them materialize. David Leary, Dean of the School of Arts and Science at the University of Richmond, gave us all the help we asked for. Students from the Classical Studies Department—Patrick Beasom, Dallas DeForest, and Baird Kistner—helped out with registration and logistics, and Phoebe Drews came to Richmond for the colloquium weekend to take care of details that she knew her husband was likely to ignore. I also thank Kathy Turner and her associates at the Virginia Museum of Fine Arts for hosting Professor Renfrew's public lecture and for the reception that followed in the Classical Galleries of the VMFA.

From the beginning, Stuart Wheeler and I recognized that only through publication would the presentations and criticism at our colloquium reach the audience that they deserve. Early in 1999 I began corresponding with Edgar Polomé about the possibility of publishing the colloquium in the monograph series of the *Journal of Indo-European Studies*, and that venerable and gracious scholar was most encouraging in his responses. Our colloquium was saddened by the announcement that Edgar Polomé had died in the preceding week. His prodigious scholarship, his work with *JIES*, and his generous help to so many people in Indo-European studies and in wider circles of the academy will be deeply missed. So far as our colloquium was concerned, we were very fortunate that Roger Pearson, James Mallory, and Richard Diebold quickly stepped into the breach and were no less interested than Professor Polomé had been in publishing the volume in the *JIES* monograph series. We are most grateful to them for taking on this project.

The eight presentations made orally at Richmond appear here in a quite different form. The papers brought to the colloquium by three of the four scholars dealing with questions of language were too long to read in forty-five minutes, and we are all grateful to the editors of the *JIES* monograph series that the full papers can be included here. Contrarily, Kuniholm's presentation was geared so closely to slides that only a brief reworking of it, to which he has added some very helpful bibliography, survives in the pages published in this volume. Barber and Zimansky read papers that were illustrated by slides and have revised their texts to compensate for the omission of the visual aids. Like Kuniholm, Renfrew spoke from notes and with slides, and in the intervening months he has devoted time and effort—for which we are again in his debt—to write for this publication a paper that not only incorporates his reaction to some of the ideas and criticism generated at the colloquium, but also utilizes an argument from genetics recently published by M. R. Richards and B. Sykes. Our two respondents were likewise given the opportunity to read the final versions of the papers they were asked to criticize, and both Melchert and Rutter have slightly revised the responses they gave last March.

My own contribution, "Greater Anatolia: Proto-Anatolian, Proto-Indo-Hittite, and Beyond," was, of course, written well after the colloquium: it is an attempt to pull together some conclusions from the papers and discussions and to fill in some gaps. Finally, references to the presence or absence of evidence for horses in Neolithic Anatolia suggested to me that a survey of the evidence would be helpful and appropriate to this volume. Fortunately, the same thought occurred to Geoffrey Summers, who attended the colloquium and has long been at work at Kerkenes, east of Ankara. Summers agreed to compile a brief survey, and his submission appears on pages 285–292 of this volume as an appendix.

Unlike the papers and responses, the text of the three discussion sessions represents quite exactly what was said at the colloquium. These sessions were transcribed directly from the archived broadcasts and the digital audio tapes made by Kenny Morrell and Walt Stevenson, and although I have edited out some asides and false starts, I have wherever possible let the extemporized comments appear as they were given, in all their spontaneity. There may be, accordingly, some disjunctures between these discussion transcripts and the published papers, but I do not think that this results in any major confusion.

In editing this volume I was fortunate to have many hours of expert technical assistance from Bard and Sherrye Young. I also thank my home department, the Department of Classical Studies at Vanderbilt University, for assisting me in many ways. The financial support from the National Endowment for the Humanities grant to the University of Richmond has been indispensable in making this publication possible. My greatest debt, finally, is to the scholars who contributed their time, energy, and intelligence to the colloquium and to the papers published in this volume.

OPENING REMARKS

by ROBERT DREWS

(following welcome by David Leary, Dean, School of Arts and Sciences, the University of Richmond)

Thank you, Dean Leary, for your gracious welcome and for your help and encouragement along the way. And I am sure that everyone here joins me in thanking the University of Richmond and the National Endowment for the Humanities for the financial support that made this colloquium possible. More personally, for work done and work still to be done let me thank our fine student assistants—Patrick Beasom, Dallas DeForest, and Baird Kistner—and the wonderfully efficient secretary of the Department of Classical Studies, Stella Jones. And a special thanks to Stuart Wheeler, chair of Classics here at Richmond, for creating this opportunity in the first place and for indispensable assistance and good judgment in organizing it. Finally and obviously, we are all indebted to the eight scholars who will be making presentations today and tomorrow and to our two critical respondents.

This colloquium addresses a topic of extraordinary interest. But, as this little room immediately indicates, most people do not yet know that. Earlier this week, hundreds of scholars gathered in Portland for the American Oriental Society's annual meeting, and two weeks from today hundreds more will be in Knoxville for the annual meeting of the Classical Association of the Midwest and South. Even the annual Indo-Europeanist conference in May will be, in contrast to ours, a mass gathering. Ours is not an annual reunion or convention. The fifty-some of us who are here have, obviously, no organization, no journal, and no history at all. We are at the beginning of something, and it is not at all clear what that thing will be. We are on the horizon of new perspectives, each of which invites exploration—so, a tiny colloquium, improvised and somewhat ragged, but an exciting intellectual moment.

Our colloquium represents an intersection of two remarkably unpopular theories. For a long time, espousing either one was sufficient to raise questions about one's credentials and intelligence. Among linguists, the Indo-Hittite theory has been out of favor almost since Edgar Sturtevant formulated it some sixty years ago. Briefly, Sturtevant argued that the Anatolian languages were not descended from Proto-Indo-European but that Proto-Anatolian and Proto-Indo-European were sisters, both descended from a still earlier language, which he called Indo-Hittite. In 1965 in his *Encyclopaedia Britannica* article on Indo-European, Joshua Whatmough noted Sturtevant's odd view, observing that "this view Sturtevant held to the end of his life, though no other Indo-Europeanist of repute now maintains it. Hence the term Indo-Hittite, introduced by Sturtevant, should be abandoned."[1] And so, for a while, and except for Warren Cowgill, it was. Philip Baldi aptly summarized the situation in 1983: "The Indo-Hittite hypothesis is very much out of vogue today in the United States (it never caught on at all in Europe); those American specialists who favored the theory seemed to lose their enthusiasm after Sturtevant's death in the mid-1950s."[2] But Indo-Hittitism was never quite extinguished. Late in the 1980s it began to pop up in odd places, and now has a respectable number of supporters, including—for the first time—several in Europe. Our colloquium is the first, I think, in which the Indo-Hittite thesis is no longer an outcast but the guest of honor.

As for the "Greater Anatolia" part of the conference, two hundred years ago there was some sympathy for the idea that Proto-Indo-European came from Greater Anatolia, and specifically from the northern slopes of Mt. Ararat. But among critical scholars an Anatolian origin of our language family was not argued until 1973, when Colin Renfrew advanced it in a brief article that went largely unnoticed. In 1985 Thomas Gamkrelidze and Vyacheslav Ivanov created a stir with their 1,100-page argument that Proto-Indo-European originated in eastern Anatolia. Another commotion broke out two years later, when Professor Renfrew extended his western Anatolian proposal into a book. Even so, the attention that the two Anatolian proposals have received has more often than not been unfavorable, and the adjectives usually applied to the idea of any Anatolian *Urheimat* have been "unorthodox" and "eccentric." I haven't polled our ten presenters and respondents, but looking over the eight abstracts I would guess that the majority—even here at the Greater Anatolia Colloquium!—is not yet persuaded that our language family originated in Greater Anatolia. And of course even the few scholars in favor have obvious disagreements about where in Greater Anatolia the place of origin should be located. Professors Renfrew and Ivanov are both right here, within six feet of each other, yet philosophically still a thousand miles apart. But there is much to be said for both ends of the Anatolian connection—much more, in fact, than has thus far been said, and I thank Professors Renfrew and Ivanov for coming here to give the Anatolian candidacy a vigorous push.

The two themes of this conference—the Indo-Hittite thesis, and the thesis that our language family originated in Greater Anatolia—should be mutually helpful, and the enlivening of the one is very likely to promote the other. It is especially curious, then, that only now are the two finally appearing in combination. When Sturtevant made his radical proposal in the 1930s, he did not suggest that the people who spoke Proto-Indo-Hittite lived in Anatolia. Instead, he seems to have assumed, as did everyone else at the time, that one or more branches of the Proto-Indo-Hittite nation migrated to Anatolia from somewhere in the north. Vice versa, when Colin Renfrew, Vyacheslav Ivanov, and Thomas Gamkrelidze made their proposals that Anatolia was the place from which our languages came, the *Ursprache* they had in mind was not Proto-Indo-Hittite but Proto-Indo-European.

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[1] Joshua Whatmough, "Indo-European," *Encyclopaedia Britannica* (1965), s.v.

[2] Philip Baldi, *Introduction to the Indo-European Languages* (Carbondale: Southern Illinois University Press, 1983), 154.

So today, for the first time, our Colloquium puts the two theses, these two pariahs, together, and we shall see what comes of it.

I have had the privilege of glancing at rough drafts of all the papers and am excited about their quality and about the range and variety of arguments that will be presented here. I am also excited about the potential for hugely worthwhile discussion, interaction, debate, and possibly—years down the road—agreement. I do not agree with all of the papers, and in fact with a bit of effort I can find something to disagree with in each one. And I can assure you that among the presenters will be clear and sometimes major disagreements, as there will be between presenters and respondents. But I am also quite sure that even from papers that strike you as wrong-headed you will learn something useful. We are at a point at which our scholarly interests will be served by keeping an open mind.

This should be a wonderful conference, a small conference, but it will be published and will still be influencing scholarly opinion when meetings many times our size have been forgotten. I hope that some day we will look back at this colloquium with satisfaction, not just as that funny little conference in Richmond, but as an important first step in reconfiguring what we know, and how we think, about our language and our history.

THE CLUES IN THE CLOTHES

SOME INDEPENDENT EVIDENCE FOR THE MOVEMENTS OF FAMILIES

E. J. W. Barber
Occidental College

Fashion is fleeting, but basic notions of clothing are very conservative. Most of us wear basic types of clothing that come straight down from the Bronze Age.

Indeed, it was between roughly 4000 and 2000 B.C., in the Late Neolithic and Early Bronze Ages, that producing cloth and clothing became the most time-consuming industry of the ancient world, beyond even food production. Early Neolithic peoples usually represented themselves naked; but as society splintered slowly into haves and have-nots with the concerted quest for metals, the haves (and eventually the have-nots) began to distinguish themselves with outward signs of rank and station, a tradition continuing unbroken to the present. Clothing became second in importance only to language for daily communication of human messages.

Precisely because fibers and their products are so perishable, until recently archaeologists seldom took cloth and clothing into account in setting up theories, including those about Indo-European or Indo-Hittite relatedness and expansion. But new scientific methods of recovering, identifying, and preserving fibers and other biological remains make it increasingly possible to wrestle information from such shreds. Thus, the data from this source have the refreshing advantage of newness: they can offer largely independent opinions on old debates, such as the place of Anatolia in the development of the Indo-European and/or Indo-Hittite cultures and languages. To preserve that independence of voice, I take no a priori stance on the position of Anatolian as sister or daughter to Proto-Indo-European, so I will temporarily nickname our undifferentiated Neolithic ancestors "proto-Us." (They are, after all, *our* linguistic ancestors.) I wish to show without preconceived biases, if possible, where this line of textile evidence leads. For the same reason, I begin without commitment to *geographical* origins, beyond assuming that we start somewhere in Eurasia. We can decide afterwards where we have ended up.

One other caveat. Because a term like "Indo-European" is the result of a *linguistic* construct, archaeology is of value to Indo-European studies—for dating, geography, etc.—only to the extent that it can be tied somehow to *language* data. That task is so difficult that too many people ignore the requirement. Quite by accident, however, I have found cloth to be a field in which such ties often occur.

* * *

The only words for clothing that we can reconstruct for Proto-Indo-European (or "proto-Us") are ones that mean "what one wears" or "to put on or wear something," such as **wes-*, and "a belt or sash" or "to gird a belt on," such as **yōs-* (cf. Lane 1931). That is not much of a wardrobe. But the larger history of clothing supports this picture. The oldest proof of fiber arts currently stands at 25,000 B.C., for twisted fiber thread and twined nets found recently in the Czech Republic (Adovasio et al., 1996), followed around 20,000 B.C. by the first proof of clothing—bands, caps, and string skirts for women, as carved on some of the Gravettian Venus figures (Barber 1991, 40, 255–58; Soffer et al., 2000). These skimpy garments were of no use for either warmth or modesty: the skirts, at least, appear to have signaled the woman's status as one of child-bearing age. Changed later forms of the string skirt were still a crucial part of women's rural dress in East Europe into the twentieth century, denoting that the woman had reached puberty and/or marriage (Barber 1999b). And modern Greek still uses the old Indo-European root in its word for the garment: *zonári* (cf. Welters 1999).

So clothing evidently began not for warmth but to carry social messages. And remember that we are softies: except in truly Arctic conditions, people *inured* to the cold do not need much body clothing for warmth. Folks were only just discovering the complex joys of clothing at the end of the Neolithic. Up to that time, if you wore anything, you used a pelt or a plain square of cloth right off the loom—a so-called blanket-wrap—and held it on somewhere with a belt or sash or maybe a pin: **wes-*, **yōs-*, and maybe **per-*.

But to have a square of cloth, you need textile techniques. The first proof of true weaving comes about 7000 B.C., from the early agricultural settlements of Jarmo and Cayönü Tepesi, followed at Çatalhöyük a millennium later by a mass of increasingly sophisticated linen textiles (see Barber 1991 for early textile data).

Indeed, all early finds of cloth with identifiable fibers have turned out to be of plant-bast fibers like linen. But about 4000 B.C., we get evidence that the sheep in the northern Fertile Crescent and Anatolia were becoming woolly. Their wild ancestors, domesticated several millennia earlier, had coarsely hairy (or "kempy") coats, not woolly ones, and were useful only for meat since their fiber was structurally unspinnable. But once sheep shifted genetically to become woolly (which now appears to have happened first in southern Iran ca. 7000 B.C.: see Darden, this volume), it was worth more to keep them alive for a continuous supply of wool and milk than to kill them for a single meal (Barber 1991; Sherratt 1981). Sheep's wool has unique properties among fibers even today—for example, it is virtually the only fiber that will mat into felt. Woolly sheep, along with horses and wheels, were what made steppe nomadism a possible way of life.

Wool also revolutionized textiles because, unlike linen, it comes naturally in many shades and takes dyes well. And having differently colored fibers meant people could start to weave patterns. Colored and patterned cloth, in turn, fueled the differentiation of clothing, which was now moving beyond the string skirt to the blanket-wrap.

* * *

Blanket-wraps are the soul of versatility. Small ones can be made into kilt-like skirts or loincloths, such as Egyptian men wore. Bigger ones can be used as Odysseus used his—cloak in the daytime, blanket to roll up in at night, and shroud when you die. The garment can hang from waist, chest, shoulders, or head. (All of these can easily be illustrated from Bronze Age monuments.)

Of these possibilities, kilts were favored by both Mycenaean and Anatolian men when first depicted. The King's Gate at Hattusas shows a striped and fringed kilt in full detail. What is odd is that the fringe is not along the edge where weavers would expect it. Fringes occur automatically where the warp is cut off the loom; and *figured* stripes, like the spiraled ones on the King's Gate kilt, are almost always woven in the weft, not the warp, for technical reasons. So the fringe should be along the bottom edge, not at right angles to it.

If we look at the detailed Egyptian paintings of Aegean ambassadors from around 1475 B.C., again we see striped kilts, the designs on which are so close a match to LM IIIA pottery (and to the first piece of pattern weaving preserved in Greece itself) that these must surely be Mycenaeans (Barber 1991: 333–37; Barber 1997). Again the fringes are always on the wrong edge—not parallel with but perpendicular to the stripes (fig. 1).

I finally found the key to this puzzle in Chinese Turkestan, where some of the clothing found near Cherchen had been constructed from dozens of narrow bands of plaited wool (fig. 2; Barber 1997; Barber 1999a: 54–58 and pl. 6a–b). Clearly any loose fringes of yarn would be hanging out the *ends* of *these* stripes! As a weaver I could not imagine making stripes this way, when it is so easy just to weave them. But thinking about the probable life-style of these shepherd people, I realized they could not lug a loom around as they followed their sheep through distant pastures. But they could easily carry wool to spin and then to plait into long, narrow bands, which they could later sew together quickly to produce large cloths. For a nomadic life-style, this odd technique made sense.

But that explanation suggests that the band-kilted Hittites and Mycenaeans also were descended from people who had traditionally—and probably recently—been nomadic (or transhumant) herders. Textile bands were so important in Mycenaean

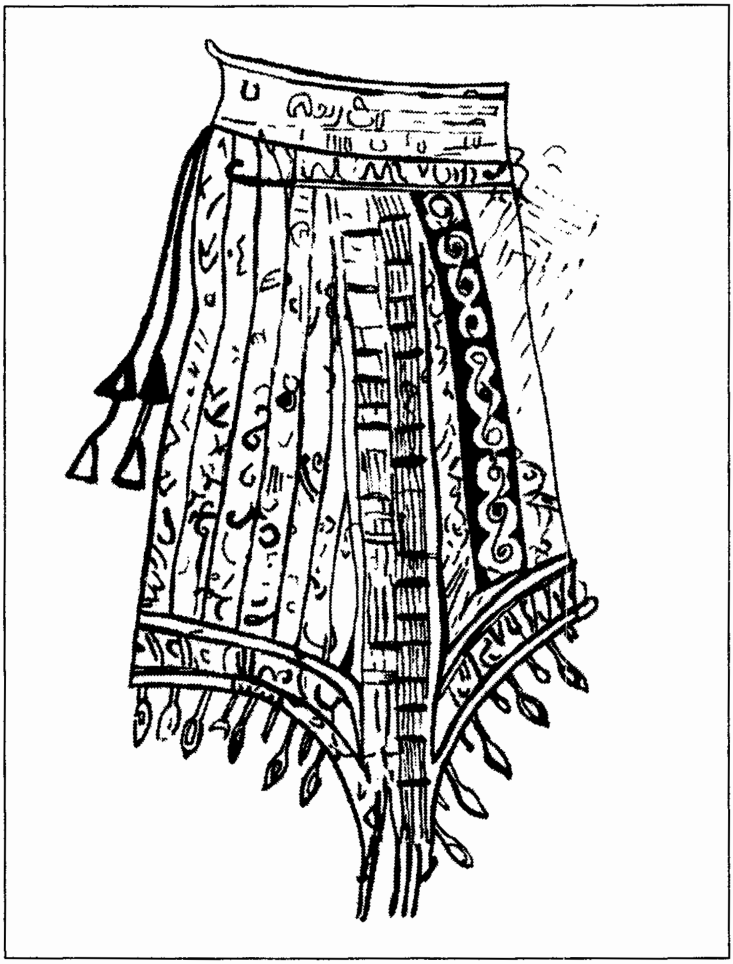

Fig. 1. Kilt worn by the leader of "Keftiu" visitors (apparently Mycenaeans) to the Egyptian pharaoh's court at Thebes, mid-fifteenth century B.C. (Traces of the oblong codpiece of an earlier Keftiu visitor, apparently a Minoan, are visible at right.) After Vercoutter 1956: pl. 19.156.

industry that the Linear B accounts give us no fewer than five words for making them (Ventris and Chadwick 1973; Barber 1991: 271–73, 312–13):

az- "warping by making a ribbed heading band"
onuk- "band with barred/ribbed pattern"
termi- "closing/warp-fringe(?) band" (= Eng. *thrum* "warp-end")
odak- "toothed(?) (plaited?) band"
ampuk- "headband"

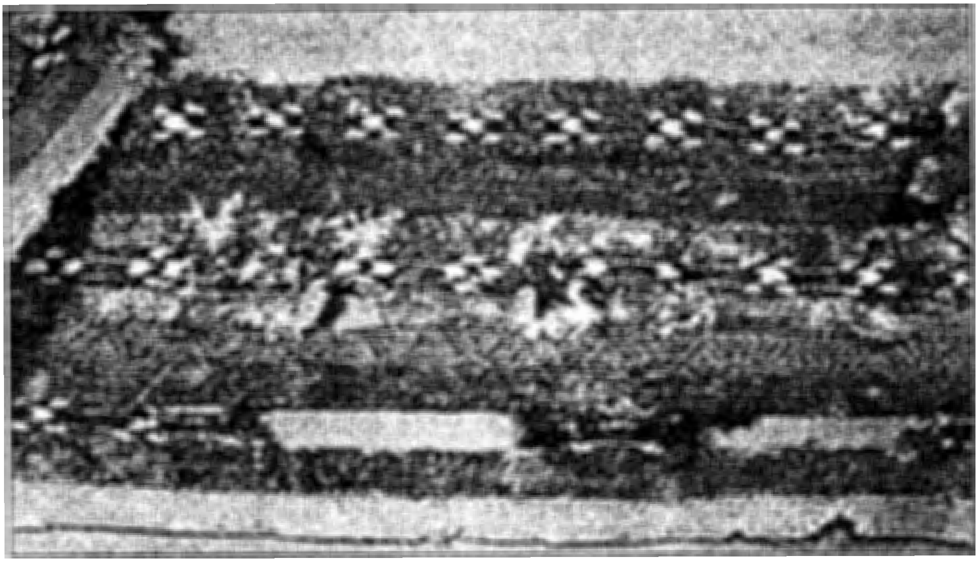

Fig. 2. Heavy striped cloth constructed from numerous narrow bands of plaited wool, from disturbed tomb at Zaghunluq, near Cherchen, Uyghur Autonomous Region; first half of first millennium B.C.

That the "proto-Us" clothing tradition had come to differ from that of other Near-Easterners can also be seen in that speakers of several "Us" languages borrowed the white linen tunic from the Semites, who had developed it in the fourth millennium. The Akkadians called it *kitinnu-*, from *kitû-* "linen, flax" (itself probably borrowed *into,* not *from,* Sumerian GIŠ.GADA "linen, flax"; Sumerian textile words are largely borrowed), the Hebrews called it *kuttoneth,* and the vowelless Ugaritic inscriptions call it *k-t-n.* Archaeological finds show that kurgan builders in the Kuban had already adopted this new fashion within the third millennium; but the borrowing was late enough that only the three Bronze Age Mediterranean neighbors got it word and all: Greek *khitṓn* (Lin. B *ki-to*), Latin *tunica* (from **ktunika,* and whence English *tunic*), and probably Hittite *kattanipu-*. (See Barber 1994: 134–38, for the development of the different shapes of this garment, including sleeves.)

Later, toward the end of the second millennium B.C. (ca. 1200 B.C.?), pants were invented—apparently to prevent chafing of tender parts as men began to ride on horseback up in the steppes. (The first surviving pairs of trousers were found at Cherchen, from ca. 1000 B.C.: Barber 1999a.) This fashion, too, spread, with each language picking up a different word. No common words can be reconstructed.

* * *

Tools to make textiles also come to light: spindles, needles, weaving equipment, dye vats, and so on. What is more, the textile tools in graves as well as the visual representations of cloth-making show uniformly that it was women who did this work, until developments late in the Bronze Age moved the making of certain prestige fabrics into men's hands. But before that, when we find textile tools of unique types moving about, we know that the women themselves were moving, since the humble sticks and lumps of clay that composed these implements had no intrinsic value for trade (Barber 1991: 283–310). Thus they give us a chance—rare in prehistory—to distinguish the migrations of family units from the movements of marauding warriors, roving cattle rustlers, or transient tinkers. Family movement, of course, spreads language much more effectively across continents than groups of men alone, since children learn their first language at their mothers' breasts. So by tracking women's work, we can sometimes get a firm grip on language itself.

The standard reconstructions of "Indo-European" terms for textile technology contain nests of words for needles and thread, spinning and sewing, linen and wool. The fact that the word from which English *wool* comes (which is semantically opposed to hair and other fibers) is attested in Hittite as *hulana*, laryngeal and all, warns us right off that Anatolian, Celtic, Germanic, Indo-Iranian, Balto-Slavic, Armenian, Greek, Latin, and the rest must still have been fairly unitary when woolly sheep began to spread rapidly up from the Fertile Crescent around 4000 B.C. Here, then, is our first strong linguistic tie to datable events: Anatolian and the other groups must not yet have parted company at roughly 4000 B.C., if indeed they lived north of, say, Baghdad.

In the reconstructions we also see weaving distinguished from plaiting within narrowly defined Indo-European (if not in Anatolian):

> WEAVE: Eng. *weave/weft/web*, Greek *huph-ain-*, Sanskrit *ubh-na-*, Tocharian *wap-*, etc.
> PLAIT: Latin *plect-*, Greek *plek-*, Old Ch. Slavic *plet-*, Sanskrit *prasna-*, (Eng. *flax* with shift of meaning to the fiber, and *plait* via French), etc.

Weaving is conceptually more sophisticated, requiring a loom and a warp vs. a weft, whereas plaiting (as in braiding hair) can be done with fingers alone using undifferentiated elements.

This distinction is not surprising, since we have proof of loom weaving from 7000 B.C. in the northern Fertile Crescent. What is surprising, however, is that words with Indo-European etymology are sufficient only for weaving on a narrow little band loom.

The simple band loom was evidently the direct ancestor, in separate but contiguous geographical areas, of all three large looms found in early western Eurasia. The horizontal ground loom developed in the Fertile Crescent, spreading via the

Levant to Egypt by 4200 B.C.; whereas the warp-weighted loom developed in Europe, first appearing definitively in the archaeology about 5500 B.C. in Hungary. (It *might* have been used at Çatalhöyük, a bit earlier, but the citable evidence is both scanty and highly ambiguous—far too weak to support any solid conclusions about origins—although the reopened excavations there may change this.) Evidence shows clearly that the warp-weighted loom spread quickly through the Balkans and up the Danube to Switzerland, where impoverished Swiss pile-dwellers were producing polychrome brocades before 2500 B.C., a full millennium before the Egyptians learned to make fancy fabrics. The Minoans, too, used this loom, refining it for weaving exquisitely patterned woolens, which they began trading in Egypt well before 2000 B.C. Far from being backward, the inhabitants of Europe were in the forefront of early textile making. (For the masses of data underlying all these summaries, see Barber 1991.)

It is a bit of a shock, then, to learn that the Indo-Europeans knew none of this. In Greek, for example, the additional terms needed for working on the European warp-weighted loom are all borrowed (except for one descriptive compound, "loom feet," for the supporting posts). In fact, Greek ended up with a double set of vocabulary for weaving: good old Indo-European words for band weaving and borrowed words for weaving wide cloth on the warp-weighted loom (see Barber 1991: 260–82 and esp. tables 12.1–4). Latin provides a similar picture; and indeed, each Indo-European language that I have looked at has borrowed or minted, independently of the other branches, all the words for whatever large loom was in use wherever that language ended up.

This means that the (linguistically defined) Proto-Indo-Europeans were *not* in the Balkans during the Neolithic when the warp-weighted loom was developed there. If they had been, the terms for this central tool would have been part of Indo-European vocabulary—or, at the very least, the western Indo-European vocabulary—before the groups parted company. Nor do they show clear evidence of living as a group in any other area where large-loom technology existed.

Of all looms, the warp-weighted loom is the easiest to track, because weights were typically made as a matched set in baked clay. We can chart the spread of this loom: west up the Danube, then northwest across Europe to Denmark in the Bronze Age, and eventually to Britain. It also spread northeast to Poland and Ukraine but never penetrated beyond Kiev and the Dnieper. In Turkey, small, pear-shaped weights associated with pattern-weaving spread eastward during the Early Bronze Age to the east edge of the central plateau, around the Taurus Mountains, then stopped. (For the hard data, see Barber 1991: 91–113, 299–303.)

Because the Greeks did not know the warp-weighted loom until well after they had become differentiated as Greeks, they must have spent at least the Neolithic living east of a line running from the Dnieper to the Taurus. Because they *adopted* the warp-weighted loom somewhere, using it in their eventual homeland, Greece, and because the warp-weighted loom is the most tiring large loom to use, they must not have had a large loom of their own already. (If you already have a large loom, why would you borrow one that is *harder* to use and no more versatile?) This suggestion also fits with the lack of vocabulary for a "proto-Us" large loom in the other languages. So perhaps the "proto-Us" homeland lay not only *east* of the warp-weighted loom but *north* of the other large looms. The ground loom is attested quite early in Syria and the Levant, Mesopotamia, and Egypt; so the homeland must have been north of that area.

And here we come up against our ignorance. The third loom, the vertical two-beam loom or tapestry loom, reached Egypt from Syria, where evidence suggests that it first appeared around 2400 B.C., possibly from yet farther north. In a zone stretching a couple hundred miles both north and south of the Caucasus, which I will nickname "Caucasia," we know from scattered artifacts that the weavers here were quite innovative between about 3200 and 2500 B.C., producing the first known examples of plaid, twill, tablet weaving, and possibly tapestry (Barber 1991: 121–22, 166–70). But as yet we have no direct knowledge of what large loom they used.

The implication is that *if* the Syrians invented the third big loom themselves around 2400 B.C., the "loom-challenged" proto-Us/Proto-Indo-Hittites/Proto-Indo-Europeans could have been sitting anywhere north of northern Syria, whether in eastern Anatolia and Armenia, or in the Kuban and the steppes east of the Dnieper. But if Syria got the loom from "Caucasia," then the Proto-Indo-European homeland (at least) must have lain yet farther north.

None of this, of course, precludes the possibility that any *cousins* of the Proto-Indo-Europeans that might have separated from the PIE branch considerably before the end of the Neolithic may have used or even invented the warp-weighted loom farther west.

* * *

The geographic picture painted by tracking spindle whorls instead of loom parts is remarkably different—though not necessarily incompatible. A spindle whorl is a humble little fly-wheel attached to the spindle to keep it twirling longer. The whorl does not have to be of any particular shape—a potato will do. But most whorls are simple variations of disks or spheres, made quite handily out of clay.

One type of prehistoric whorl, however, was neither disk nor sphere, but a scooped-out bowl or nose-cone shape (fig. 3); and at sites where this curious shape occurs, the excavators invariably remark that they have never seen so many spindle whorls in their lives. (For the data, see Barber 1991: 303–10). Troy level II, for example, disgorged 10,000 of them; and they poured from Late Bronze Age sites in the Po valley so profusely that the designers of the 1884 Exposition at Turin twined long strings of them around the columns in the exhibition hall, to both store and display them (Wilson 1893–94: 968).

The puzzle of the large numbers of whorls is the easiest to solve. Most modern spinners remove the new yarn from the spindle when it is full. If you do not, you

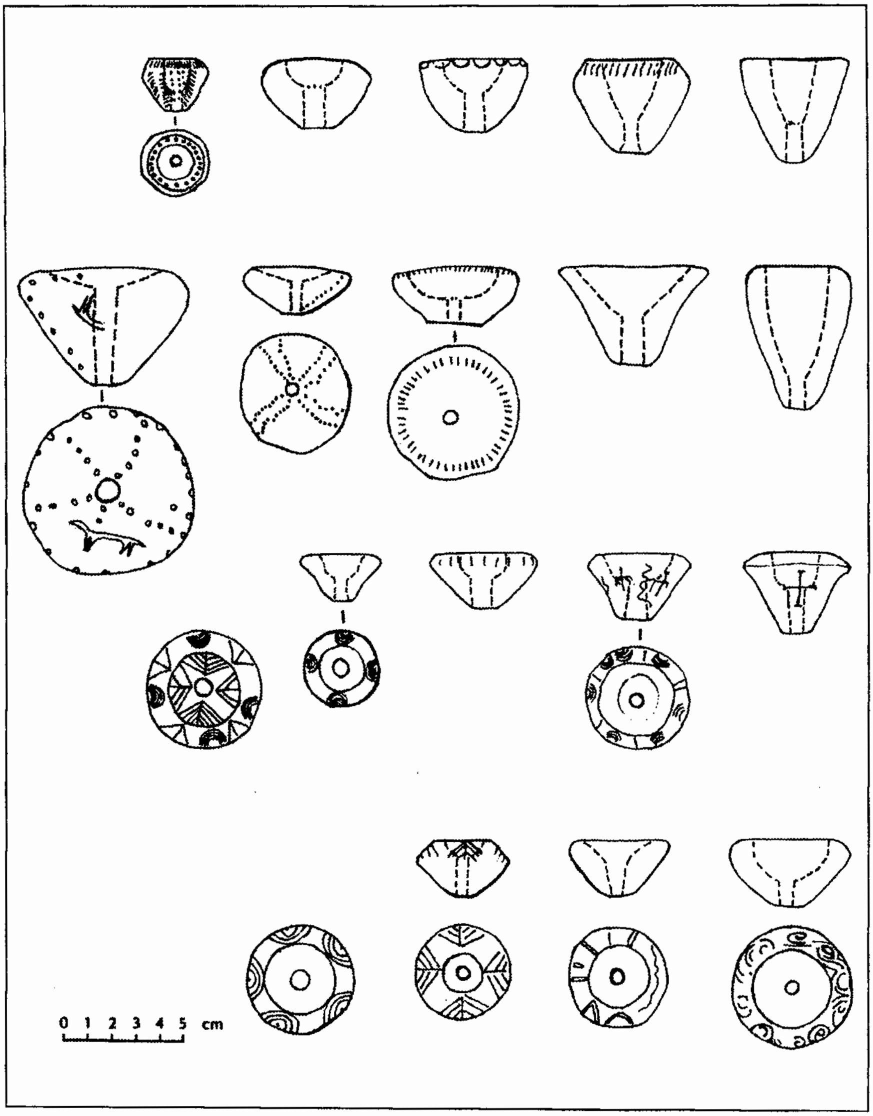

Fig. 3. Hollow spindle-whorls from several Neolithic and Bronze Age sites. Top row: Anau (Turkmenistan), levels I-II. 2d row: far left, Rajki (Ukraine); next 2, Ćmielów (SE Poland); right 2, Gródek Nadbużny (SE Poland). 3rd row: Anau, level III. 4th row: Troy (found by Schliemann; like whorls found by Blegen in levels I-III). Oddly, the Rajki whorl appears to have been used the other way up from the Trojan and Anau whorls with stick-figures. (After Pumpelly 1908: 163; Barber 1991: 307; Pumpelly 1908: 165; Schliemann 1875/1968: pls. 22, 33, 40, 31.)

will need another spindle, and another, and another, to keep on making thread. And it takes a lot of spindles-full to weave a piece of cloth.

If the thread is still on the spindle when one is ready to weave, a whorl heavy enough to spin wool or linen will be too wide to pass between the threads on the loom. But you could have the same weight whorl without the *breadth* by curving the clay into a bowl or nose-cone shape—and the cone would even help guide the spindle through the shed on the loom.

So it seems these Bronze Age women had redesigned their equipment to weave right off the spindle. Why might they have wanted to weave directly from the spindle?

Maybe they had not yet figured out how to keep the spun yarn from untwisting once they let go of it, so they had to weave it straight into the cloth. Or perhaps they were nomadic, with plenty of time to spin as they herded their sheep but very little opportunity to set up a loom and weave. They could use the spindles like lace-bobbins to plait bands directly; or, with a pouch full of loaded spindles, they could be ready to make the most of what sedentary time they had to weave.

The archetypical hollow whorls occur at Anau, in Turkmenistan, already in the first Neolithic levels there. So the founders of this odd site on the north side of the Kopet Dagh must have arrived from the Iranian side of the mountains already using quantities of hollow whorls, most probably for the wool of woolly sheep. Shapes and decoration continued to develop through levels I, II, and III. My attempts to find yet earlier ones in the direction from which these colonists came, however, are frustrated in particular by insecure dating of so many Neolithic sites, including the levels at Anau. You cannot set up trustworthy archaeological vectors until you know for sure what came before what! But if indeed woolly sheep first developed in southern Iran (see Darden, this volume), Iran should be the direction in which to search.

If we set aside how and when this strange tradition *reached* Anau, we can still pick out eddies of its later movements, some of which look uncannily familiar. Indeed, the map I published a decade ago in *Prehistoric Textiles* (Barber 1991: 306, fig. 14.6) continues to show how intriguing the movement of these whorls is—and how rich a thesis topic for someone to pursue, as new and better dates roll in from fieldwork.

In the late Neolithic we can trace the copious hollow whorls slithering along the outer fringes of the Tripolye C2 culture, northwest across the Ukrainian steppes into southern Poland—as though the bearers might have wandered up from east of the Caspian (following their flocks in a wet year?). Then, at the start of the Early Bronze Age, about 3000 B.C., they arrived in force at Troy, in level I, and started spreading south and east across Anatolia—a vector suggesting they reached Troy from the north, across the Sea of Marmara. That in turn suggests that those families heading west across the Ukraine had relatives—not yet located—skirting westward around the Black Sea. (Perhaps the new excavation sites of 3000 B.C. off the shore of Bulgaria will produce hollow whorls.)

Thus the whorls imply that the founders of Troy came from the north, families and all, and that their relatives then infiltrated western Anatolia from the northwest. Decades ago, when I was studying Hittite with Albrecht Goetze at Yale, that great scholar used to say that he felt the Luwians must have been living in western Anatolia long before the Neshite Hittites took over from the east (with their Hurrian-influenced ways) to organize the lot of them into the Hittite kingdom. One of Goetze's arguments was that whenever the Neshites wanted to claim the full weight of their authority to rule, they started talking Luwian. Even the great royal seals are bilingual. Perhaps the whorls show us these "Proto-Luwians" arriving.

But the plot gets thicker. In Troy level II, back at 2600 B.C., where Schliemann alone found 8,000 of the peculiar whorls, we see decorations uncannily similar to those at Anau III, 2,000 miles to the east, as though there were some pipeline between them.

Meanwhile, the strange whorls found a beachhead at Lerna just as the House of Tiles went up in flames and was enigmatically entombed in its own big round tumulus (or kurgan)—this somewhat before 2000 B.C., at the close of EH II. Such whorls became much more common in Greece during the Middle Bronze Age (who might we suppose brought them, if not, perchance, the first trickles of Greeks?), at the same time that loom weights became much less common for a while, as though the indigenous users of the warp-weighted loom had become diluted by a population of non-users. And who might that have been? We have just seen that the Greeks learned of and borrowed all their vocabulary for this important tool *after* they parted company from their fellow Indo-Europeans. The fit now is like that of the wallpaper to the wall.

The hollow whorls died out in Greece at the end of Middle Bronze—only to turn up in the Late Bronze Age in northern Italy. At first I thought that the carriers of these whorls might have wandered through Greece and on to Italy. But the Middle Bronze Greek whorls look like tired versions of the once crisp tradition, whereas the Late Bronze Age whorls in Northern Italy and Switzerland look as if they came fresh out of another pipeline from Anau III!

These odd whorls occur so often in places where later inscriptions reveal Indo-European and/or Anatolian speakers living that I think their movements are worth some serious attention. But the picture will stay hazy until we can fill some of the holes in the data.

* * *

The later we go, the more likely the cloth itself is to survive. For example, my book on the mummies of Ürümchi (Barber 1999a) shows photos of plaid twills of 1200–700 B.C. from Chinese Turkestan next to plaid twills of 1200–400 B.C. from Hallstatt, Austria, where the immediate ancestors of the Celts then lived. When I trekked to Ürümchi in 1995, I sincerely doubted any Celtic connection. I had discovered earlier (Barber 1991) that twill was the favorite weave in Bronze Age Europe (unlike in the Near East, where a different weave had been invented to do the same job, namely cope with wool). But how hard is it to think up stripes?

After I had sifted all available evidence, however, it became clear (1) that although stripes were widespread in the Bronze Age, few thought of weaving stripes *both* directions, and almost no one made those stripes of rhythmically uneven widths, so as to create a plaid rather than a checkerboard; and (2) that the two groups shared some rather odd, non-functional habits (namely, weaving dark-ground plaids specifically in twill, but white-ground cloth with red and blue pinstripes specifically in plain-weave). Because both cross-stripes and twill (as I mentioned above) are first attested in "Caucasia" between 3200 and 2500 B.C., the simplest explanation for the striking parallels was that this weaving tradition had been carried westwards from "Caucasia" by the Celts just when it was being carried eastwards from there by . . . by . . .

Yes, there *was* a group of Indo-Europeans known (from texts dating from the sixth to the ninth centuries) to have made it to Chinese Turkestan: the Tocharians. Furthermore, Tocharian is said to share many dialect features with, of all things, Celtic. How convenient! But can we close a gap of 1,000 years between the first Tocharian writings and the plaid twills, and another 1,500-year gap between the plaid twills of 1000 B.C. and those of "Caucasia" at 2500?

The Tocharian to Bronze Age gap shrinks easily. The Chinese historical records of local events and place names show that Tocharians indeed lived in the northeastern Tarim Basin well before 110 B.C., when the Chinese arrived in force (Barber 1999a: 119–24, 220).

The other gap is harder to fill. The canonical view of Indo-European dispersal requires that the Tocharians headed east sometime between 3000 and 2000 B.C. But the plaid twills do not turn up in Xinjiang until about 1200 B.C. If the Tocharians brought the plaid twills, where were they living in the meantime? Who knows? Maybe they set up the Afanasievo culture, then decided it was just too cold in Siberia and headed on south to Hami. (Someone needs to go look at the bottoms of the Afanasievo pots for traces of twill weave; plaid, of course, won't show up in impressions unless the yarns differed in some way besides color.)

When they left home is another question. What Tocharian and Celtic (and Italic) *share* dialectally is chiefly archaisms, not innovations. That implies that these groups headed off on their own before anyone else except the Anatolians (who seem to preserve even more archaisms). Since the requisite textile technology was in use by 2500 or even 3000 B.C., they *could* have left then.

But wait: both plaids and twills almost necessarily resulted from weaving wool on large looms; and our earliest plaid—from a third millennium kurgan on the Fars River just north of the Caucasus—is huge. That suggests that some of the first non-Anatolian Indo-Europeans to get up and leave, namely the Celts and Tocharians, must already have been using a large two-beam loom of some sort, despite the lack of identifiable vocabulary for it. So although the ubiquitous *borrowings* for the warp-weighted loom still show that the Indo-Europeans (narrowly defined) started from east of that loom line, a north/south stricture—postulated on the *lack* of specific terms—will not hold.

* * *

So what have we got?

The reconstructed clothing terms match the archaeology in showing a very simple Early Bronze Age wardrobe—an all-purpose blanket-wrap with a belt and perhaps a pin—until a few westerly subgroups borrow the linen tunic, word and all, from the Semites. Hittite and Mycenaean men, however, favored a kilt curiously formed from narrow bands, in a way recently elucidated by the finds from Cherchen among Caucasians who, as I have shown elsewhere, were probably of Iranian descent (Barber 1999a: 159–66). The unusualness of this technology, apparently stemming from the needs of nomadism, shows strong cultural and geographic ties between these linguistic groups. And the pervasive borrowing of terms for the warp-weighted loom and its technology, which developed in central Europe early in the Neolithic, demonstrates that the Proto-Indo-Europeans could not have lived west of the Dnieper or eastern Anatolia in the Neolithic.

And because the Anatolians and the "other" Indo-Europeans all share their special term for a central commodity that was not biologically available in the north before about 4000 B.C.—namely wool—they presumably did not expand before then. Woolly sheep, along with wheeled vehicles, were what made steppe nomadism possible.

The discovery of a highly idiosyncratic technology of plaid twills among the ancestors of the Celts in Hallstatt, Austria, 1200–400 B.C., and among the apparent ancestors of the Tocharians near Hami in Chinese Turkestan, 1200–700 B.C., not only corroborates the linguistic indicators that these two groups were among the earliest to leave the "homeland," but also points to the area I nicknamed "Caucasia" as the point of departure, since that is where *both* those weaves seem to have originated around 3000 B.C.

Finally, late in the Neolithic and all during the Bronze Age, the migration of some humble yet strange little spindle whorls—used only by women and thus indicating the movement of entire families—suggests groups of wanderers moving into territories which presently turn out to contain Anatolian, Mycenaean, and other linguistic groups descended from "proto-Us." The earliest such whorls turn up in, of all places, Neolithic Anau, where settlers had just arrived, sheep and all, from northwestern Iran. Who were these settlers, and have they anything to do with our problems? Only more excavation in Iran, Turkmenistan, and southern Russia will solve this puzzle.

All of this textile material suggests strongly that of all Anatolia only the easternmost part could have been involved in the formation of Proto-Indo-European, and only as the southern portion of a more northerly-centered "Caucasia." Western Anatolia, like Europe west of the Dnieper, seems ruled out for the stage linguistically defined as Proto-Indo-European. Such a scenario is not entirely incongruent with the model currently suggested by Renfrew (this volume), if we equate his first stage with a "proto-Us" that spawned a western branch (largely lost to us linguistically) that spread with the first farmers of Europe, and an eastern branch (Indo-Hittite) that presently divided into Anatolian speakers (who rattled about in Anatolia and around the Black Sea in ways not yet fully understood) and Indo-European speakers who sojourned a while in (northern?) "Caucasia" before expanding to their classical locations.

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ARCHAEOLOGICAL INQUIRIES INTO ETHNO-LINGUISTIC DIVERSITY IN URARTU

Paul Zimansky

In comparison to neighboring lands to the south and west, literacy came late to Eastern Anatolia. The few Hittite and Mesopotamian references to places and peoples of the region prior to the first millennium B.C. are fraught with ambiguities and uncertainties not always recognized by scholars making brief forays into the area to demonstrate one point or another. When the ruling dynasty of Biainili, known to the Assyrians and other neighbors as Urarṭu, finally brought the area into the realm of "cuneiform culture" toward the end of the ninth century, it did so on a very limited basis.[1] The texts that survive are almost exclusively royal, the great majority being rather formulaic display inscriptions reporting conquests, building activities, and cultic endowments. While they offer an impressive number of geographical names defining the historical and geographical parameters of the kingdom, they are not particularly useful in helping to understand its internal structure. Linguistically the texts are also impoverished; although they are quite pronounceable thanks to the employment of a standard cuneiform script with a high frequency of syllabograms, their simple declarative sentences in the first or third person and the limited vocabulary involved in presenting the restricted subject matter give scant opportunity for tracing substrates and borrowings.

This first episode of native literacy lasted only a little more than a century and a half and then ran dry when the empire that engendered it was overthrown. Linguistic, historical, and artistic traditions in later periods show little consciousness of,[2] or continuity with,[3] Urartian conventions. Nevertheless, the priority that this manifestly non-Indo-European language has in the written record of the lands between the Taurus, Zagros, and Caucasus has had a profound influence on perceptions of the linguistic prehistory of the area and been taken as a key datum in discussions of Indo-European migrations. The general perception that the Hurrian language, closely related to Urartian, moved into northern Syria and Mesopotamia from this area in the late third and second millennia B.C. and a putative affiliation of Hurro-Urartian with languages still spoken in the Caucasus[4] bolster the notion that this was the dominant language family in the region before representatives of the Indo-European family arrived.

It is through an examination of the character of this Hurro-Urartian linguistic area in the early first millennium B.C. that I believe I can make a contribution to the theme of this symposium, although I recognize that my time period postdates the period of central concern to most of the participants and that my specialization in Urartian archaeology is a rather remote observation point from which to study linguistic changes. The basic point argued here, however, may well be applicable to other eras and perhaps other locations: In an area like eastern Anatolia, apparent cultural uniformity may be imposed rapidly in such a way as to mask linguistic diversity, and there were many interstices within the Urartian empire in which Indo-Europeans may have lurked. We know from a number of sources that there were a multiplicity of different languages spoken in Urartu, but when we go looking for them archaeologically, we find only Urartians.

When and under what circumstances did Indo-European, and specifically Armenian, become a major player in the region of eastern Anatolia that textual evidence suggests was dominated by Hurro-Urartian dialects in the Bronze and Iron Ages? In her admirable overview of Armenian history, A. E. Redgate has framed the issue as follows:

> The chronological question [in dating the Armenian migration] is important for on it depends the validity of treating Urartian history as a significant part of Armenian history rather than as merely a background to its beginning. Were the Armenians in the sixth century BC a relatively primitive people, rats who had deserted the sinking ship of the short-lived Phrygian Empire for firmer ground in the seventh century BC, or were they an advanced people, who had participated in the culture of the recently extinguished Urartian Empire?[5]

The theory that Armenian migrations into eastern Anatolia took place after the fall of Urartu, toward the end of the seventh century B.C. or the beginning of the sixth, still has its adherents,[6] though it is now less popular than the view that Armenians were already on the scene much earlier. The most thorough study of the problem is I. M. Diakonoff's *Predhystorija Armjanskogo Naroda*, subsequently updated by the author and translated into English.[7] His study reviews the classical sources for the relationship of the Phyrgians and Armenians and essentially posits an Armenian presence within Muški or slightly to the east of it in the early Iron Age. Muški is generally, if not universally, equated with the kingdom of Phrygia ruled from Gordion, on the early history of which recent excavations have shed some light.[8] Diakonoff makes the following statement:

> Beginning with the middle of the 8th to the end of the 7th centuries B.C., the entire territory from the mountains of the Cilician Taurus to the east was engulfed by mighty empires which could hardly have allowed significant

> ethnic movements through their territories. And in any event such movement could hardly have taken place without information about it being preserved by the numerous chronicles, annals, inscriptions, and royal letters which have come down to us.[9]

I believe that we do indeed have adequate grounds for positing ethnic movements within the "mighty empire" of Urartu, and that this kingdom itself was almost certainly polyglot and ethnically pluralistic. It is a mistake to regard the Urartian empire as a cultural barrier to the spread of Indo-European languages. I will present my arguments for this position first by reviewing points I have made before on the formation and structure or the Urartian state,[10] and then turning to new evidence that is coming out of the ground at Ayanis, on the east shore of Lake Van, where I am currently working as part of the excavation team directed by Altan Çilingiroğlu of Ege University.

Preliminary Considerations

The parts of eastern Anatolia that came to be dominated by the Kingdom of Van are centered around some of the most thinly settled areas of modern Turkey. The physical reasons for the dearth of inhabitants are not hard to discern. The country is divided by intersecting chains of mountains in a way that frustrates direct long-distance movements in almost all directions. It contains small areas of alluvial land that are quite productive when brought under irrigation, but when the region is viewed via modern satellite photography, one is stuck by how far removed those areas are from one another and how much of the territory is desolate. Less than 10 percent is under cultivation. The great lakes of the region, Van and Urmia, are both useless economically because their waters are too alkaline and saline, respectively, to be potable or usable for irrigation and are lethal to fish.

Another factor that distinguishes this region is the severity of the winters, particularly the depth and duration of the snow cover. In fact, the part of Turkey covered by snow more than eighty days per year approximates rather well the territory of the Urartian state. The mountain passes that control communications between the various islands of sedentism can close as early as late September and not reopen until May.[11]

These conditions lent themselves to insularity, low populations, and a dependence on transhumance. It is hardly surprising that the area has not been distinguished by high cultural achievement except in a few rather circumscribed instances. The Kingdom of Urartu was one of these, and I have argued that its emergence and survival was fostered by the defensive potential of this mountainous landscape in the face of a perennial threat from Assyria.[12]

Although there is a great deal of room for research and a single major discovery could revolutionize our understanding, the lack of evidence for significant sedentary populations in the centuries preceding the rise of Urartu is striking. In the Late Bronze Age, some would locate the kingdom of Azzi-Hayaša in the area of Erzurum and Erzincan, but this state is known almost exclusively from Hittite epigraphic sources—specifically a well-preserved treaty—in which it does not appear to be at all centralized or coherent.[13] Archaeologically, there is nothing to go on but some pottery that seems to have links with the Caucasus. There are also thirteenth-century references to a geographical area called Uraṭri or Uruaṭri, which appears to have been divided among a number of kingdoms.[14] The fact that this name is cognate to Urartu, the term that the Assyrians later applied to the Kingdom of Van, has led many to conclude that such things as confederations of ethnic Urartians, or the Urartian language, dominated the mountains to the north of Assyria in the Bronze Age, but I see no solid evidence to support this. Nor are the proper names containing the theophoric element Haldi found in Middle Assyrian texts proof of the existence of Urartians in this era: the names are otherwise in good Akkadian, and we shall see that the god Haldi was not associated with the Urartian state at the outset.[15]

In the Van region in particular, the thirteenth through ninth centuries are characterized by an almost complete absence of settlement. There is some evidence from cemeteries such as Dilkaya and Karagündüz, indicating that the land was by no means depopulated, and the general assumption is that it was dominated by small, relatively mobile social groups.[16] This is not, incidentally, at all unusual for the early Iron Age in much of Anatolia and Greece.

The Formation of the Urartian State

The chronology of Urartu's rise is quite securely outlined by Urartian and Assyrian documents, the former providing the geographical information and the latter a series of synchronisms for the temporal scaffolding. Van was not the first place the Assyrians confronted an organized power in Urartu, but it was where the dynasty that was to control Urartu in its heyday established its power. That dynasty appears to have come from somewhere else, and all the indicators point to a location farther south, in the knot of mountains around Rowanduz.[17] The attraction the plain of Van offered seems almost certainly to have been its defensive potential and its remoteness from Assyria. The mile-long citadel rock, next to which Sarduri I built the first known Urartian structure, is a natural fortress and the Urartians limited the amount of it they needed to defend by carving two large breaks in its spine. The distance between Van and Nimrud may be short as a very energetic crow might fly, but the mountains one must cross are virtually impenetrable, even today.

Once the new dynasty set itself up in Van, its armies immediately began adding territory to the kingdom. The first addition was the region north of the lake, but soon thereafter Išpuini and Minua added the western side of the Urmia basin to their holdings. A recently found inscription in Nakhichevan[18] shows that Išpuini went much farther north than we had previously imagined, and it may well be that the activities of his grandson, Argišti I, in the rich plain of Ararat, involved development rather than conquest of new territory. Minua's inscriptions testify that he had crossed the Euphrates in the west. In short, the distribution of inscriptions shows that the Urartian state expanded very rapidly at the end of the ninth century and perhaps through the first two decades of the eighth. There seems to have been very little significant expansion of Urartian territory thereafter. This empire was created by military conquest pure and simple and is unlikely to have involved movements of large numbers of people at the outset.

The subsequent history of Urartu is another matter. The two surviving sets of royal annals make it clear that one of the duties of an eighth-century Urartian king was to lead the armies in one or more campaigns annually. These were directed against the pastoral societies of eastern Anatolia, and since lists of Urartian booty trumpet the numbers of POWs and quantities of livestock captured above all else,[19] it would appear that the objective of these conquests was the acquisition of manpower and the wherewithal to feed it, rather than territory or loot in other forms. Although we have no formal annals for the seventh century, foundation inscriptions of Rusa II make it clear that these policies continued until nearly the end of Urartian history.

The Urartian "State Assemblage" and the Nature of Urartian "Culture"

In the process of building their empire, the Urartians introduced an abrupt transformation in the material culture of the areas they took over. It is not just one of scale but also of kind. The emergence, expansion, and, eventually, collapse of royal authority are linked, in this case, to a distinctive assemblage with an intimacy rarely seen in the archaeology of states or empires. Without clear local antecedents, new types of pottery, architecture, and metalworking appear shortly after the capital of the kingdom was created at Van and spread outward from there in lockstep with Urartian armies and administrators.

Nothing , however, is mysterious about this correlation. The Urartian government was deliberately transforming the cultural landscape and imposing its own ideologies and material expression upon it. Preexisting manifestations of elite culture, such as the center at Hasanlu, were obliterated or transformed as areas were integrated into the restructured hierarchy. The focus on state-produced materials is also somewhat heightened by modern archaeological priorities in the area, which have emphasized excavation of fortresses—prima facie creations of governments. Since there is only one, highly centralized, government in this case, the rapid spread of a single standardized assemblage is to be expected.

Let me illustrate this overwhelming central control with two examples. First, there is the evidence of inscriptions. Most of the texts we actually have are royal compositions, framed in the king's name. Even the most mundane administrative tablets tend to be sealed with the king's seal and contain decrees in the king's name.[20]

Writing, it would appear, was very nearly a royal monopoly. It did not exist in the region before the Urartian kings conquered it and disappeared when they did. I once attempted an analysis of Urartian personal names and found that very few could be confidently identified. The texts mention lots of enemies but almost no Urartians. Even the kings are suspect: the name of Rusa was probably unpronounceable in Urartian, being the only term in the language to begin with an initial *r*. One can get a few personal names out of the very limited corpus of clay tablets, but otherwise the written record gives the impression that there was never more than one Urartian acting at a given moment.

A state religion also appears to have been fostered by the Urartian monarchy and to have shared its fortunes. The chief god of the pantheon, Haldi, existed before the kingdom but was very obscure. It was the second Urartian king, Išpuini, who elevated him to supremacy over such notables as Teišeba, the storm god.[21] The Urartians invented a quite distinctive temple form, square in plan with reinforced corners, which is found only on Urartian citadels. Although claims have been made of a relationship with tower temples in the Levant, the resemblance is remote.[22] The plan may have influenced Achaemenian tombs,[23] but as a place of worship it disappeared with the kings at Van.

The writing system, state religion, and other canons of the Urartian assemblage, were probably created by choices of a very small number of individuals in a short period of time. One mechanism by which a tiny elite can propagate a uniform assemblage over an extensive geographical area is offered by the Inca empire. Its rulers set up a system of way stations and administrative centers reminiscent of those of the Urartians, who were also great road builders. Craig Morris has remarked on this phenomenon in the Inca civilization:

> Since it is probable that nearly all of the productive activities in these centers were directly related to the state, it would have assumed nearly total responsibility to provide housing, pottery and all else that was needed. The pervasive nature of the state stamp on everything down to the crudest cooking pot would tend to affirm that the state lived up to its obligations with almost unbelievable rigidity.[24]

The enclaves share many characteristics of Urartian centers, including the rapidity with which they were created, the emphasis on storage, and rapid abandonment when the empire was decapitated. Morris notes that the very people who might create and employ the state assemblage in an Inca center would use completely different, local materials when they returned to their villages.[25] It seems to me that the Urartian assemblage is analogous to "Inca" materials in the empire of Tahuantinsuyu: a set of items used by peoples when acting directly under the authority of the central government.

The Contributions of Ayanis

The research project in which I am currently engaged was designed to explore this relationship between the state assemblage and population of the empire by investigating the material culture of the outer town surrounding the fortress of Ayanis. The site lies about thirty-eight kilometers north-northeast of Van, its citadel lying about a kilometer inland on a natural eminence commanding a view of the lake. A Turkish team from Ege University led by Altan A. Çilingiroğlu has been excavating the fortress since 1989, discovering a foundation text built into the city gate in 1994 that provided the ancient name of the site, "Rusahinili below Mt. Eiduru", and its founder, Rusa II.[26] It is thus one of an extraordinary group of centers created by the last significant king of Urartu, who reigned from at least as early as 673 B.C. until shortly after 651 B.C. The citadel is surrounded by a well-dressed wall and entered through a monumental gateway. It contains numerous pithos-storerooms and has a large courtyard with basalt pylons. This is not the appropriate place to catalogue the exciting discoveries the Ege University has been making season after season, but I cannot resist mentioning one artifact, as it has resolved a longstanding philological debate. When the Urartian kings went to battle, they marched out with a *šuri*. German philologists of an earlier generation tended to regard this as a chariot, and Russians thought it was a weapon, perhaps a sword, on the basis of an Armenian cognate. In 1997 the Ayanis team unearthed a ceremonial lance about eighty centimeters long that is unquestionably a *šuri*–it says so right on the handle.[27]

In 1996 Elizabeth Stone and I joined the Turkish team and began a systematic investigation of the outer town through shovel testing, magnetic field gradient survey, and soundings in selected areas. Our goal was to determine how the urban environment was organized and to uncover assemblages from various places within it for comparison with the materials coming from the citadel. Thus, we hoped to see how thoroughly the "state assemblage" permeated the society associated with a major center late in Urartu's history. We were able to establish relatively quickly that at least eighty hectares of settled area surrounded the fortress and that there was a certain amount of internal differentiation, judging by the relative percentages of ware types that we found on survey.

Our work, which has been generously supported by the National Geographic Society, was just getting under way in earnest when a major inscription containing information directly relevant to determining who lived in the town was found on the facade of a temple on the citadel. It is the longest seventh-century Urartian text yet discovered—a nearly complete version of what might be termed the "standard inscription" of Rusa II, previously known only in fragments from several other sites[28]—and gives unprecedented detail on royal building projects. A significant and unique passage in the last two lines on the right side of the temple entrance states, "Rusa the son of Argišti speaks, 'I conquered the enemy lands: the land of Assur, the land of Targu, the land of Etiuni, the land of Tabal, the land of Qairanu, the land of Hatti, the Land of Muški and the land of Siluquni.'" It then goes on to say that he settled these people here, in the palace and the "cities," by which he presumably means the settled areas around Ayanis.

Targu and Qairanu cannot be identified, but the location of the others is reasonably well known. Assyria, which was preoccupied with a rebellion in Babylonia in 651 B.C., the year tree rings suggest the building at Ayanis was completed, was hitherto assumed to be on friendly terms with Urartu. Tabal, around modern Kaiseri, is far to the west, as are Hatti, which in this era means Carchemish and northern Syria, and Muški, that is, Phrygia. Etiuni is a general term for areas north of Urartu, probably stretching to the Kura Valley. Siluquni can almost certainly be equated with Suluqu, probably to be located south of Lake Sevan, in view of the discovery of an Urartian inscription near Sisian mentioning it.[29] In short, people came from foreign lands all around Urartu's territory and included Indo-European speakers as well as other groups in a mixture that must have made Ayanis an extraordinarily cosmopolitan place. Muški and Tabal, incidentally, figure prominently in discussions of early homelands of Armenians by Diakonoff and others.

It is not news that the Urartians were deporting populations in much the same way that the Assyrians did and that the Babylonians were to do later. This inscription, however, shows that this was going on in the last years of Urartu's power, and it also suggests these movements were on a larger scale than we had any reason to believe earlier.

Rusa had to move a lot of people around because of the lavishness of his building projects. Previous Urartian kings seem to have been satisfied with one major center, such as Sarduri II's Sardurihinili at Çavuştepe, but Rusa II built at least five.[30] Within sight of Van, he constructed what may have served as a new capital for the state at Toprakkale, naming it after himself, Rusahinili.[31] A tablet found at that site by the Lehman-Haupt expedition of 1898 lists 5,507 people as palace personnel.[32] He built the richest Urartian site yet excavated, Karmir-blur, in the vicinity of Erevan, again within sight of an existing Urartian center, the citadel of Erebuni, providing it with numerous storerooms sufficient to supply a small army. The high citatel of Kef Kalesi, near Adilcevaz on the north shore of Lake Van, was also no mean project. It included a large palatial structure with basalt pylons and notable sculpture. It is, in fact the only Urartian site at which reliefs comparable to those from Assyrian palaces have been found. The largest of his projects was the massive site of Bastam, excavated by the German Archaeological Institute from 1968 to 1978. The citadel stretches along a ridge for almost a kilometer, at the foot of which is a settlement area and a huge rectangular enclosure that may have been an army camp. Rusa named Bastam "Rusa's small city," which may have been his idea of a joke.

All this building demanded thousands of man-years of labor, to say nothing of the manpower requirements of garrisoning and maintaining the new fortress/settlement complexes. The Ayanis inscription suggests that many of these human resources came from outside the kingdom, and given that Rusa II was the last

Urartian king of any consequence, they did not have long to assimilate before the empire was swept away.

How did this diverse population manifest itself archaeologically? The outer town at Ayanis appears to be a one-period occupation created at the same time as the citadel and abandoned when the latter was put to the torch. We find the imprint of the Urartian state assemblage everywhere in the settlement, despite variations in its character from area to area. For example, the highly polished red ware that is so specific to seventh-century Urartian sites was found everywhere in the outer town but in the highest percentages on the higher ground of the slope to the east of the citadel known as Güney Tepe. On the flat low-lying areas west of the citadel, high percentages of coarse wares and very few red ware shards were found. In the middle elevations, including a terrace to the north of the citadel known as Pınarbaşı, medium and coarse shards were dominant, and there were modest percentages of red polished wares. But all of the pottery is essentially Urartian, and we have yet to find, for example, a single painted shard of the style of pottery that was in favor at Alişar and other central Anatolian sites at this time.

Our magnetic field gradient survey has produced very clear architectural signatures in different areas. In Pınarbaşı there are several large, planned structures that covered most of the area flat enough for residence. On Güney Tepe the picture is somewhat more mixed. There are very substantial independent houses arrayed along the contours of the hill that suggest the presence of elites. Between them are other, less well executed structures that seem to have been created after the initial occupation. Their walls are not invariably grounded on bedrock, as is the Urartian practice, and the stone construction of the wall footings is variable and irregular. These buildings, large and small, were relatively clean, but in the passageways between them was a mass of refuse, including fragments of luxury goods and a clay bulla inscribed with an individual's name. On low ground to the citadel's south, in what appears to be the largest building outside the citadel with two-meter thick walls, we excavated a kitchen area with rather humble cooking pots. The animal bones have not yet been analyzed in detail, but it is clear there are significant differences between the areas. Cattle and horses, for example, are much more prominent in the garbage of Güney Tepe than they are in Pınarbaşı, where sheep and goat predominate, and it would appear that people higher up the slope enjoyed better cuts of meat.

All of this makes it clear that the people who were settled around, and supported, the fortress at Ayanis did not enjoy equal access to wealth or a uniform material assemblage. But whatever the differences in status between the residents of the two areas, they all made use of artifacts that we would consider Urartian.

Whether there were, in fact, any residents of Urartu not associated with such centers as Ayanis remains to be demonstrated one way or another archaeologically. There is textual evidence of scattered minor settlements in the eighth century in the famous account of Sargon's eighth campaign (714 B.C.), in which an Assyrian army marched through a part of Urartu and found scores of "settlements in the vicinity" of each fortified enclave. At Ayanis we have looked for these and not found them, but of course we cannot be sure that we have adequate means of detecting them if they were, for instance, composed of tents.

One final note on the Ayanis settlement, and other Urartian settlements, that may be relevant to the thesis I am advancing: it was not burned and appears to have been abandoned peacefully. This is in sharp contrast to the citadel, where evidence of violent destruction is everywhere. Every one of Rusa's great centers shows the same pattern: the settlements are clean and, frankly, rather unexciting to dig, while the fortresses suffered a conflagration. Little evidence exists in either the fortresses or settlements to suggest that many people felt Urartu was worth dying for in its final hour.

Conclusions

Let me now return to the issue of Indo-European migrations. The Urartian state established itself in an underpopulated area in which there were transhumant groups but virtually nothing in the way of sedentary communities, at least in the western portions of their territorial holdings. There is no way of knowing whether or not the Armenians were already in the area when the empire was formed. The Urartians themselves may not have been at all numerous, and their ruling dynasty appears to have moved north into the Van area from the mountainous region east of Lake Urmia—what came to be known slightly later as Hubuškia. With their military strength and talent for defensive architecture, Urartian kings introduced irrigation, rounded up populations and their livestock, and, if not initially then certainly by the seventh century, settled them in fortress communities. Within these communities they imposed their own standards and norms of material culture to the extent that indicators of other ethnic groups simply disappear—even in cases like Ayanis and Argišthinili where texts tell us these other groups were present. It should be noted that this imposed material culture is not something that had a strong existence before the Urartian state itself was created and did not survive its demise. It serves, in this instance, as a mask that hides the true ethno-linguistic diversity of the empire.

During the second quarter of the seventh century, the infrastructure of the country appears to have been transformed by Rusa II, who created at least five new centers of extraordinary size and complexity, most of which were in close proximity to older citadels. More obviously than at earlier royal enclaves, large settlement areas appeared beside them. All of the evidence suggests that this construction and restructuring involved substantial movements of people, many of whom were being brought in from the west. The Ayanis inscription shows that the very places to which earlier scholars like Manandian and Diakonoff looked for the Armenians were those places whose populations were deported and settled in the Van region. Thus, far from being a barrier to Indo-European migrations, the Urartian Empire may well have served as their facilitator. But Rusa died sometime between 650 and 640 B.C., less than a decade after the construction of Ayanis was completed, and the kingdom went up in flames shortly thereafter. There was no time for assimilation and consolidation. Deprived of ideologies and controlling forces that had brought them there, the inhabitants of eastern Anatolia were at liberty to forge a new identity.

With regard to the larger issue of the Indo-European homeland, the Urartian episode can serve as a warning that under certain circumstances it does not take many people to change the material culture of a thinly populated environment if they are in control of the dominant institutions. Conditions under which language change might be part of this imposition may readily be imagined, but in an entity as ephemeral and unusual as the Kingdom of Van, they are unlikely to have been operative.

Notes

1. The phrase is from Gernot Wilhelm, "Urartu als Region der Keilschrift-Kultur," 95–113 in *Das Reich Urartu: Ein altorientalischer Staat im 1. Jahrtausend v. Chr.*, ed. Volkert Haas; Konstanzer Althistorische Vorträge und Forschungen; Xenia, Heft 17 (Konstanz: Universitätsverlag Konstanz, 1986).

2. Perhaps the most telling evidence of discontinuity is the utter ignorance of Urartu displayed by the early Armenian historian Moses of Khorene, who attributed the most conspicuous surviving Urartian textual and mortuary monuments, carved into the citadel rock at Van, to the Assyrian queen Semiramis. See R. W. Thompson, *Moses Khorenats'i, History of the Armenians,* trans. and comm. Harvard Armenian Texts and Studies 4 (Cambridge, Mass. and London, 1978), 98–101. C. Tuplin, "On the Track of the Ten Thousand" *Revue des Études Anciennnes* 101, no. 3/4 (1999): 360-61 has recently questioned whether the old hypothesis that the terms *Χαλδοι* and *Χαλδαιοι* of Classical sources refer to followers of the head of the Urartian pantheon, Haldi, might not have more merit than it is generally accorded today. Cf. P. Zimansky, "Xenophon and the Urartian Legacy," in *Dans les pas de dix mille* (*Pallas* 43), ed. Pierre Briant (Toulouse: 1995), 261–62. I cannot rule out the possibility that later peoples claiming the legacy of the defunct empire adopted or were given this name, but I remain skeptical.

3. For an overview of later references to Urartians, see Amir Harrak, "The Survival of the Urartian People," *Bulletin of the Canadian Society for Mesopotamian Studies* 25 (May 1993): 43–49.

4. I. M. Diakonoff and V. Starostin, *Hurro-Urartian as an Eastern Caucasian Language*, Münchner Studien Sprachwissenschaft, Beiheft 12 (Munich: R. Kitsinger, 1986). Cf. R. Smeets, "On Hurro-Urartian as an Eastern Caucasian Language," *Bibliotheca Orientalis* 46 (1989): 259–79.

5. A. E. Redgate, *The Armenians* (Oxford: Blackwell, 1999), 14.

6. E.g., Charles Burney and David M. Lang, *The Peoples of the Hills* (London: Weidenfeld & Nicholson, 1971), 173, 177–80.

7. I. M. Diakonoff, *The Pre-History of the Armenian People,* trans. Lori Jennings, (Delmar, N.Y.: Caravan Books, 1984), first published as *Predystorija armjanskogo naroda,* (Erevan, Izdatel'stvo Akademii Nauk Armjanskoi SSR, 1968).

8. Robert C. Henrickson and Mary Voigt, "The Early Iron Age at Godion: The Evidence from the Yassıhöyük Stratigraphic Sequence," in *Thracians and Phrygians: Problems of Parallelism*, ed. Numan Tuna et al. (Ankara: Middle East Technical University, 1998), 79–106.

9. Diakonoff, *Pre-History of the Armenian People*, 110.

10. These are presented more extensively in my *Ecology and Empire: The Structure of the Urartian State:* Studies in Ancient Oriental Civilization 41 (Chicago: Oriental Institute of the University of Chicago, 1985).

11. In recent years the Van area has been subject to a severe drought and the snowcap of Turkey's second highest peak, Mt. Süphan, all but disappeared in August, 2000. Villagers in Ağartı, who are used to coping with meters of snow in the winter, say that there was only "two centimeters" last year, and the whole agricultural system is threatened by lack of water. This is regarded by all as highly unusual.

12.This is the essential "thesis" of Zimansky, *Ecology and Empire*.

13. Jak Yakar, "Beyond the Borders of the Hittite Empire," in *Hittite and Other Anatolian Studies in Honour of Sedat Alp*, ed. H. Otten et al. (Ankara: Türk Tarih Kurumu, 1992), 511 argues for a location in the plain of Erzurum. For a recent translation of the treaty, see Gary Beckman, *Hittite Diplomatic Texts*, SBL Writings from the Ancient World, vol. 7 (Atlanta: Scholars Press, 1996), 22–30. G. B. Djahukian, "Did Armenians Live in Asia Minor before the Twelfth Century B.C.?" 25–31 in *When Worlds Collide: Indo-Europeans and Pre-Indo-Europeans*, ed. T. L. Markey and John A. C. Greppin (Ann Arbor, Mich.: Karoma, 1990) argues that Armenian names can be found in the Hittite texts relating to this area and that "the presence of Armenian language elements can be explained only by the presence of Armenian tribes (26).

14. A. K. Grayson, *Assyrian Royal Inscriptions, Volume 1: From the Beginning to Ashur-resha-ishi I* (Wiesbaden: Otto Harrassowitz, 1972), 81.

15. M. Salvini, "La formation de l'état urartéen," *Hethitica* 8 (1987): 402–6.

16. Veli Sevin and Ersin Kavaklı, *Van/Karagündüz: An Early Iron Age Cemetery* (Istanbul: Akeoloji ve Sanat Yayınları, 1996), 51.

17. Mirjo Salvini, "La storia della regione in epoca Urartea," in *Tra lo Zagros e l'Urmia: Ricerhe storiche ed archaeologiché nell' Azerbaigian Iraniano*, ed. P. E. Pecorella and M. Salvini (Rome: Ateneo, 1984), 17–18.

18. S. G. Hmayakyan, V. A. Igumnov, and H. H. Karagyozyan, "An Urartian Inscription from Ojasar-Ilandagh, Nakhichevan," *Studi Micenei ed Egeo-Anatolici* 38 (1996): 139–51.

19. For a summary of Urartian booty lists, see Zimansky, *Ecology and Empire*, 58.

20. The scarcity of Urartian tablets is puzzling. I. M. Diakonoff, *Uratrskie pis'ma i dokumenty* (Moscow and Leningrad: Akademia Nauk SSSR, 1963) published all of the examples known at the time, amounting to a mere sixteen texts. Since then several more have been found at Bastam and Ayanis, but the total remains around two dozen, contrasting with the hundreds of dedicatory inscriptions on stone and metal objects. Scores of bullae with brief cuneiform notations written on them from seventh-century sites do suggest that cuneiform writing was widely used for administrative purposes, so it may be that the paucity of tablets is simply due to mischance in discorvery.

21. Mirjo Salvini, "Le panthéon de l'Urartu et le fondement de l'état," *Studi epigrafici e linguistici sul Vicino Oriente antico* 6 (1989): 79–89.

22. David Ussishkin, "On the Architectural Origin of the Urartian Standard Temples," *Tel Aviv* 21 (1994): 144–55.

23. David Stronach, "Urartian and Archaemenian Tower Temples," *Journal of Near Eastern Studies* 26 (1967): 278–88.

24. Craig Morris, "State Settlements in Tawantinsuyu: A Strategy of Compulsory Urbanism," in *Contemporary Archaeology*, ed. M. P. Leone (Carbondale: Southern Illinois University, 1972), 399.

25. Craig Morris and D.Thompson, "Huanuco Viejo: An Inca Administrative Center," *American Antiquity* 35 (1970): 359.

26. Altan A. Çilingiroğlu and Mirjo Salvini, "'Rusahinili in Front of Mount Eiduru': The Urartian Fortress of Ayanis (seventh-century B.C.)," *Studi Micenei ed Egeo-Anatolici* 35 (1995): 111–24.

27. A. A. Çilingiroğlu, *Urartu Krallığı: Tarihi ve Sanatı* (Izmir: Yapım-Organizasyon Universal A.Ş., 1998), 148, res. 92–93.

28. An announcement of the discovery of the inscription and a discussion of its context appears in Altan A. Çilingiroğlu and Haluk Sağlamtimur, "Van-Ayanıs Urartu kalesi kazılari, 1997," *XX. Kazı Sonuçları Toplantısı*, vol. 1 (Ankara: Kültür Bakanlığı Milli Kütüphane Basımevi, 1999), 528–29. An edition of the text is currently being prepared by Çilingiroğlu and Mirjo Salvini.

29. N. V. Arutjunjan, "La nouvelle inscription urartéene découverte en Arménie soviétique," 89–93 in *Gesellschaft und Kultur im alten Vorderasien*, ed. Horst Klengel (Berlin: Akademie Verlag, 1982).

30. P. Zimansky, "An Urartian Ozymandias," *The Biblical Archaeologist* 58, no. 2 (June 1995): 94–100.

31. Unambiguous proof that it was Rusa II rather than Rusa I who founded Toprakkale is lacking, but a bulla fround at Bastam, which we know was created by Rusa II, is dated to the year in which Haldi "set up the throne in Rusahinili Qilbanikai." This strongly suggests a change of status, if not foundation, for Toprakkale in the reign of Rusa II. The bulla is published in M. Salvini in Wolfram Kleiss, ed. *Bastam II: Ausgrabungen in den urartäischen Anlagen 1977–1978*, Teheraner Forschungen 5 (Berlin: Gbr. Mann, 1988), 130–38.

32. Diakonoff, *Urartskie pis'ma i dokumenty*, no. 12, 38–39, 80–83.

DENDROCHRONOLOGICAL PERSPECTIVES ON GREATER ANATOLIA AND THE INDO-HITTITE LANGUAGE FAMILY

Peter Ian Kuniholm

When Professor Drews kindly invited me to participate in the Indo-European/Hittite Colloquium at the University of Richmond last March to speak on "Pinning Down the Date of the Black Sea Inundation," I was not at all sure how that title fitted into the theme of the meeting. Since my tree-ring work is not at all based on linguistics, I felt I was superfluous to such a discussion. But having listened to the various papers, I think I might now be able to offer a few appropriate or even useful comments.

The comment I made in Richmond was that, regardless of when and how the Bosporus opened, it was and is not an obstacle to anybody's passage, Indo-European-speaking or otherwise. I have swum it in twenty-three minutes, and my students used to do it in thirteen. Our wiggle-matched radiocarbon dates for Early Bronze Age (EBA) Kiten, today some ten meters under water on the Bulgarian coast, show that there were people living on what is now the Black Sea seabed in the years just preceding 2700 B.C. or some 2,900 years after the circa 5600 B.C. date proposed in the popular press for the splitting open of the Bosporus and the saline incursion into what had been a freshwater lake. (See P. I. Kuniholm, B. Kromer, S. L. Tarter, and C. B. Griggs, "An Early Bronze Age Settlement at Sozopol, near Burgas, Bulgaria," in *James Harvey Gaul: In Memoriam*, ed. M. Stefanovich, H. Todorova, H. Hauptmann [Sofia: James Harvey Gaul Foundation, 1998], 399–409; also the December 1998 Aegean Dendrochronology Progress Report at <http://www.arts.cornell.edu/dendro>).

The recent hyperbole in the media about the salinization of the Black Sea and its putative association with "Noah's Flood" has not helped anybody toward a clearer understanding and appreciation of what is otherwise a straightforward geological argument. The clearest archaeological exposition of the probable splitting open of the Bosporus sometime around the Early Bronze Age is by Mehmet Özdoğan, "Marmara Bölgesinde Kültür Tarihi ile İlgili Bazı Sorunlar ve Bunların Çözümüne Jeomorfoloji Araştırmalarının Katkısı," in *I. Arkeometri Sonuçları Toplantısı* (Ankara: Eski Eserler ve Müzeler Genel Müdürlüğü, 1985), 139–62; followed by "The Black Sea, the Sea of Marmara, and Bronze Age Archaeology: An

Archaeological Predicament," written for a festschrift for Henrieta Todorova (of which I have seen only the typescript). Our wiggle-matched dendrochronological date for Kiten fits in nicely with Özdoğan's theory. However, my feeling on the splitting open of the Bosporus is that the exact date is not really relevant to any dispersal of any language.

Now for some comments on the theme of the Richmond meeting: What strikes me is the essential futility of linguistic versus archaeological arguments, considered independently of one another. The archaeologists of early periods are rarely if ever in a position to make informed judgments on the linguistics that are relevant to their trenches. The linguists, similarly, are rarely in a position to make informed judgments on the material culture of the civilizations whose languages they study or the precise date and duration of those civilizations. The exceptions might be some of the excavators of Hittite and Mycenaean sites or team members competent in the languages.

Professor Renfrew's valiant attempt to synthesize the two trains of thought by positing a homeland for IE in Anatolia, and which I suspect was why Professor Drews convened the meeting in the first place, seems to me to deserve credit rather than the grumbles that have arisen from one side or the other. Most of his critics have not informed themselves of the other half of the story.

The principal merit, I think, of Professor Renfrew's argument is its minimalist approach, no sweeps of bands or tribes across Asia and Europe, no invasions, no disasters, but rather a peaceful movement of agricultural technology over millennia. The time-scale, then, is what is significant for all of us. Secondly, a small band of PIE people, however prolific, speaking to one another cannot begin to account for the totality of the IE dispersal over the entire Eurasian landmass. One has to have a significant body or bodies of people in addition to the passage of many thousands of years. As I suggested in Richmond, Anatolia is an especially attractive place of IE origin simply because of the vast range of dates and the number of the settled communities on record. Manfred Korfmann, Ayşe Baykal-Seeher, and Sinan Kılıç's *Anatolien in der Frühen und Mittleren Bronzezeit* (Beihefte zum Tübinger Atlas des vorderen Orients, Reihe B [Geisteswissenschaften] Nr. 73/1 [Wiesbaden 1994]) provides a catalogue of 1,520 excavated or surveyed settlements in Anatolia, some of them quite large, covering the period of only ca. 3000–2000 B.C.

Part of the work of the Aegean Dendrochronology Project, as reported in Richmond, has been to fix in time the occupation of some of these Anatolian sites during the millennia before the EBA. Our date list in reverse chronological order included the following sites (details for each are not important but rather the multi-millennial chronological range):

Kültepe, Eski Sarayı = Karum II, circa 2033 B.C.
Acemhöyük Early, between 2687 and 2185 B.C.
Demircihüyük and Kiten, just before and after 2700 B.C.
Troy I, a pit in the Schliemann Trench, circa 2718 B.C.
Arslantepe, Level VIA (Late Uruk), circa 3374 B.C.

Sürmene/Ağaçbaşı, circa 4251 B.C.
Niğde/Köşk Höyük, ending circa 4820 B.C.
Hacılar, ending circa 5400 B.C.
Can Hasan, ending circa 5600 B.C.
Çatalhöyük, ending circa 6449 b.c.
Aceramic Aşıklı Höyük, throughout the 8th millennium B.C.
Basal Aşıklı Höyük, circa 7955 B.C.

This listing of dates is based on long tree-ring sequences, many of them wiggle-matched by radiocarbon. And long before any of these settlements were inhabited, new excavations in the Urfa region—notably at Hallan Çemi in the eleventh millennium B.C. (the wood of which we have not yet been able to use for dendrochronology) and the extraordinary ninth-millennium-B.C. sites excavated by the German Archaeological Institute—show that significant settlement and domestication of both plants and animals was going on at a time that would have been quite unthinkable to most archaeologists just a generation ago. The excavator of Hallan Çemi, Professor Michael Rosenberg, tells me that he thinks there are settlements in Anatolia significantly older than his by a thousand years or more, simply because Hallan Çemi shows clear signs of urban development. Using whatever terminology one wants, Epipalaeolithic or Early Neolithic, one sees that Anatolia has as respectable and long a record of human settlement as anywhere else in Eurasia. For a recent, well-illustrated compendium of the latest work on the early periods of settled communities, see *Neolithic in Turkey: The Cradle of Civilization*, ed. Mehmet Özdoğan and Nezih Başgelen (Istanbul: Arkeoloji ve Sanat Yayınları) 1999, 2 vols., text and plates.

Specifically, where other than Anatolia do we have such a collection of so many settled people so early? What one needs to ask of the linguists is, Are 9,000 years enough for the Indo-European languages to develop and spread so that one can explain the already-major differences between Hittite and Mycenaean in the second millennium B.C.? I should think so. Until a similarly large body of people spread out over such a long period of time can be identified, it seems to me that Professor Renfrew is on to the beginning of an answer.

Note

For additional bibliographic references to the work of the Aegean Dendrochronology Project see our Website at <http://www.arts.cornell.edu/dendro>. For readers without access to a computer, see P. I. Kuniholm, "The Prehistoric Aegean: Dendrochronological Progress as of 1995," *Acta Archaeologica* 67 (1996): 327–35; also M. W. Newton and P. I. Kuniholm, "Wiggles Worth Watching—Making Radiocarbon Work: The Case of Çatal Höyük," *MELETEMATA: Studies Presented to Malcolm H. Wiener*, 3 vols. (Liège: Université de Liège, 2000), 527–36.

Discussion Session

SATURDAY MORNING

(A question, not captured on tape, pertained to the date at which people began to ride horses)

KUNIHOLM: David Anthony at Hartwick College has been looking at skeletons of horses, both broken and unbroken (to riding, that is). He has found evidence of wear from the bit back behind the molars, so that when you find a horse out on the steppes, and you see those little grooves there, then it's quite clear that somebody has been doing things to that horse and making it behave.

RENFREW: Anthony's views are being very much challenged by Marsha Levine. When you have about 90 percent horse bones, that is an indication of domestication only because they are eating the horses, not necessarily riding them.

KUNIHOLM: Yes, but if you look at the jawbones themselves, unless you are going to go chew on the horse's jaw, this must be some kind of indication of domestication.

RENFREW: But in parentheses, it's worth saying about those wonderful Çatal dates you've produced, that there are horses there. I think 8 percent of the bones at Çatalhöyük are horse bones, wild horses. Of course Asıklihöyük also has horses.

KUNIHOLM: It would be interesting to look at the Çatal horse bones to see whether there's any of this wear that Anthony talks about. It's the kind of thing one would not necessarily look for.

RENFREW: If I may, this is purely a terminological point, but I find a little confusing Elizabeth's terminology. I'm not questioning her comments or her conclusions in that context, but this "proto-Us" business. We already have this as a well-established term in "Nostratic." Now one doesn't necessarily have to believe the Nostratic hypothesis, but *Nostratic* was named by Pedersen: *nostri* = our countrymen. There is an uncomfortable adjacency between the one terminology and the other. I don't think Professor Barber is talking about Nostratic in any way, so is "proto-Us" the best choice of terminology?

BARBER: That was an attempt to be sufficiently comic to try to get people to temporarily move away from our prejudices about what is Proto-Indo-European, what is Proto-Indo-Hittite, what is Proto–Anything Else. Because it is my sense that when I, for example, read your material, if I remove the terms *Proto-Indo-European*, *Proto-Whatever*, and put in something completely noncommittal, like *proto-Us*, I suddenly see it in a new light. In other words, it allows me to get away from my prejudices. And that is what I was trying to do with that term, to look at the data, instead of being theory driven for a bit, to be data driven and to say, "What does this look like if we get away from our schooled notions of where this linguistic idea fits and where that linguistic idea fits? What does our data look like?"

RENFREW: Well, I think the strategy is an admirable one, although it's ethnocentric, and you have to be careful there, or you'll upset the Hungarians [laughter].

BARBER: But if the language that is being spoken at this conference is descended from the language of whoever these people are, that's enough, and that's all that I meant by it. The idea was to get away temporarily, for purposes of this conference, from our prejudices, because I think the data looks very different when you use some silly term that doesn't have any connotations. Then you can start to look at the data.

RUTTER: A question for Peter. Those dates that you were giving for the destruction of the Eski Sarayı at Kültepe . . .

KUNIHOLM: Not the destruction of it but actually the construction date.

RUTTER: But the tablets presumably come from shortly before the destruction . . .

KUNIHOLM: You people already know all of this anyway, but in the *kārum*, where there were found some thirteen or fourteen thousand unpublished tablets and on which an international committee is currently working, the bulk of them are from this II period. The length of the *kārum* II period has been disputed for years. They do things on the basis of *limmu* names. The *limmu* are magistrates, like archons. And until relatively recently there were seventy-one or seventy-two *limmu* names known; so it was thought that the *kārum* II period was about that long. Veenhof is working on now—and is going to publish a tablet in April—which has a complete list of 129 *limmu* names from this period, making it more than fifty years longer than it had been thought to be, and apparently there are links to the Mari chronology. There are names of kings and the like, which is really glorious. Anyway, these *limmu* and the merchants who functioned under them were doing business with people who had Indo-European names. So for whatever it's worth, in the twenty-first century B.C., there were plenty of Indo-European folks to do business with.

DARDEN: This is a comment intended for Elizabeth Barber. You mention that for the Tsarskaya plaid, you have a date of 2700 B.C. or thereabouts. There finally are some carbon-14 dates from the Maikop culture. They're buried in a footnote in a Russian edition on the Aeneolithic and Bronze Age in the Caucasus. And it seems to start about 3750 and the Tsarskaya is late Maikop. And the end dates are sort of variable. There are two ways that they do it. One estimate of the end is 2750 BC. The other looks to north of the Kuban. The Tsarskaya or Novosvobodnaya culture is replaced by the Novo-Titorovka culture at least by 3100 B.C., at least by carbon date. So it may well be that your plaid is a fourth-millennium plaid.

BARBER: That matches, actually, a little better with the earliest twill, and this is where this whole problem of date matching is so difficult. It comes from a better dated site, which is Alishar, and that is dated about 3200. So that is just about dead on. Yes, that's nice! Thank you.

IVANOV: Among documents of the Hattusha archive there are not many but are quite definitely a part that probably were rewritten from the documents that we should have searched for in Karum Kanesh, because "singers of Kanesh" are mentioned there. Nesha is mentioned there. You know that the name of the language comes from it. So my suggestion for archeologists is that, probably and still, besides the main archive of Assyrian tablets there might have been another archive. So maybe one should seriously think about the possibilities of excavating the other part of the *kārum*, that may have this archive.

KUNIHOLM: The *kārum* goes on for four and a half kilometers. And so it is not a trivial matter, and Tahsin Özgüç has been excavating it annually since 1948. But his epigrapher didn't produce the books. So now that he's retired, they are getting at these names. You get anywhere from two hundred to a thousand tablets per household. We are talking about major business. It's terrible that it has gone on so long. A generation has passed us by, and we don't know what the hell is in there, except for little snippets that have been leaked out from time to time.

LEHRMAN: I have a comment on Professor Barber's terminology. The one useful thing, I think, about using the terminology *Indo-European*, *Proto-Indo-European*, and *Proto-Indo-Hittite* is the matter of subgrouping. That is, if Proto-Indo-European and Proto-Anatolian are laterally related, then the things we could reconstruct from Proto-Indo-Hittite are very different from the ones that I see, for example, in your examples. Since the term for *belt*, PIE **yōs* (*yeHs* in your handout) is not attested in Anatolian, we therefore cannot reconstruct it for "proto-Us," but what we really mean is that we can't reconstruct it for Proto-Indo-Hittite. We also do not find the term for *weaving* in Anatolian. Hittite *hupp-* does not mean to weave; it means *to treat someone badly*, as far as my knowledge of Hittite goes. That also changes that reconstruction; that is, we cannot say with certainty that the root *web* appeared in the protolanguage of our family, that is, in Proto-Indo-Hittite. So from that point of view it's useful to keep this terminology, because it stands for something real. It stands for the realistic reconstruction, for what we can reconstruct.

BARBER: I absolutely agree with you, and all I am trying to do is to get people to back off long enough to see what the data is. I made my living for the last thirty years being a linguist. I prefer to think of myself as an archeologist, but I do not underestimate at all the power of linguistic reconstruction. But I think that we tend to accumulate so many ideas from what we've read that we need to go back and examine our assumptions. I don't intend *proto-Us* to refer to any time period, to Nostratic or Proto-Indo-European or Proto-West-Indo-European or anything else. I want a term that is so general that we say, okay, we're talking about this somewhere, but let's go back and look at the data and match it up all over again. That's what I'm trying to accomplish with that term, and it's intended to be silly, to get us out of our preconceived notions long enough to take a new look at the data and match it up again. Because, I think, when we take that new look, we see that we really have moved on in our scholarship, even over the thirty years of my scholarly career. And as Margalit Finkelberg has said in one of her articles, we talk about some of the non-Indo-European ancient languages of Europe, like the Etruscan, and so forth, as though we were still back in 1900. We haven't gone back and looked at those assumptions again. We need to go back and see what assumptions were being made then and throw them out if they no longer hold and look at the whole thing afresh. That is what I am trying to do with this silly term, and it is intended to be just as silly as possible [laughter]. It is not intended to be a serious term. It is intended to help us to back off and look again.

KUNIHOLM: Let's go back to Professor Renfrew's picture of the chap on the horse riding his way across the steppes all by himself spreading Indo-Eurpoean values as far east as India and China, and as far west as Scotland. This bothers, I think, more than just a few of us. What does it take to spread something like the Indo-European languages? It takes a lot of people, it seems to me. And in Korfmann's latest version of the Tübinger Atlas Beiheft, he has a two-page map and bibliography of the Early Bronze Age sites in Anatolia, and I believe it's 1,510 known sites. Now this is a non-trivial number of people. A lot of places, covering a lot of territory, and if you are going to have movements and so forth, fifteen hundred settlements does a lot more for my feeling of what could possibly have happened than fifteen hundred horsemen, however able they might have been. A population explosion.

One other thing: In your graph yesterday, you had a lot of things happening about 2700 B.C. Why did you pick that particular date?

RENFREW: Which graph are you referring to?

KUNIHOLM: There was one graph in which you were free-falling down through the centuries, and then there were about eight or nine things all happening right after 3000 B.C.

RENFREW: I was simply offering a proposed approximate chronology for the phases I was looking at. And I suppose I was talking about the end of "Old Europe" there.

As I explained yesterday, I was taking the Gimbutas concept of Old Europe, ending maybe 3000 rather than 2700. That's approximate, and I very much prefer dendrochronological dates to radiocarbon dates; I think it's simply the way to undertake precise chronology. In any case, the end of the Old Europe unity is around that time. And, if you are thinking of the same diagram, it would be the end of that "Old European" (using the Gimbutas terminology in some sense) *Sprachbund*, which I spoke of yesterday. And I will show that slide again in the next session.

KUNIHOLM: How chronologically convenient then. Because my argument is that until about 2700 BC the Black Sea was a fresh-water lake, in which salmon could have lived. There are still, even today, beech trees on the north slopes of the Pontus Mountains, and you have birch trees. I mean, I have cut them. I have seen the skins of sea otters hung up on the walls of foresters' cabins and the like, all of the things that we want to have for the Indo-Europeans. The material is right there, but as long as you've got the Black Sea in your mind it's rather irritating. Take the Black Sea away, and make a lake out of it, and it's a whole different game.

DREWS: So there we have it! It's 11:50 on Saturday, March 18, and we've solved it! [laughter, adjournment].

THE ANTOLIAN ORIGINS OF PROTO-INDO-EUROPEAN

AND THE AUTOCHTHONY OF THE HITTITES

Colin Renfrew

In this paper an attempt is made to consider the implications of the view that the original "homeland" of the language ancestral to all known Indo-European languages—i.e., "Proto-Indo-European" in the broadest sense (including Proto-Indo-Hittite and the pre-Proto-Indo-European proposed by some authors [e.g., Diakonoff 1990: 57])—was located in south-central Anatolia (Renfrew 1973; 1987). This may imply that the Hittite language was indigenous to Anatolia and that there is therefore no need to look for any hypothetical "arrival" of the Hittites to account for the origins of the Hittite state. As we shall see, it necessitates also a much earlier date for Proto-Indo-European than the classical view of Schrader, Childe, Gimbutas, and Mallory ("the SCGM view") would hold. In consequence, this in turn requires the recognition that, if the term *Proto-Indo-European* (PIE) be applied to all the phases of language development within the Indo-European family prior to the emergence of the various recorded languages, a long developmental process is involved. We cannot deny to the languages, for instance of central Europe in 2000 B.C., the designation *Proto-Indo-European* just because we are also using that term for the language spoken in south-central Anatolia around 7000 B.C. Yet clearly the term means very different things in each case.

The notion of time depth becomes a central one. There are different stages and different places in the story of the development of Proto-Indo-European, which can no longer be thought of as a single, localized entity. That, as we shall see, is the principal implication of the Anatolian view. It carries with it fundamental implications for the field of what is sometimes called "Indo-European studies." For it implies that there was in reality no single, unified Proto-Indo-European society. There were many Proto-Indo-European societies in different places, as Proto-Indo-European speech spread out from Anatolia (initially during the process of farming dispersal). Proto-Indo-European represents a long developmental sequence of languages over a broad range of space and time. It will be argued that, while it applies first to the early farmers of south-central Anatolia, such as those of Çatalhöyük, it applies later to the Bronze Age societies of Europe, some of which no doubt correspond more closely both in speech and in social institutions to PIE on the classical SCGM view. All of this implies that henceforward we shall have to be very much more careful in specifying *which* Proto-Indo-European society or dialect we are speaking of. The field is a broad one in space and time, and without such precision we shall remain confused by the complications and contradictions arising from a simplified, homogeneous, and standardized view of an idealised Proto-Indo-European.

The Basis for the Hypothesis of Anatolian Origins

The Anatolian viewpoint argued here has been arrived at following three strands of argumentation. My own position was reached through arguments of an essentially archaeological nature, involving the rejection of the traditional "mounted warrior nomad" theory of Schrader (1890) and Childe (1926) as reformulated in the kurgan theory of Gimbutas (1973) and Mallory (1989; see Crossland 1967), which is referred to here, as noted above, as the SCGM view. The very early dates proposed for the domestication of the horse (Anthony 1986; Anthony and Brown 1991) have now been corrected (Levine 1999) along with the realization that the horse was not ridden for military purposes until the late second millennium B.C. (Renfrew 1998b). Some decades ago I came to the view (Renfrew 1973; 1987) that the only process or event of sufficiently general significance for the whole of Europe to account for the Indo-Europeanisation of almost an entire continent was the coming of farming. As summarized below, that process, when we trace its origins back from Europe to its pre-European roots, leads one to Greece and further to south-central Anatolia, and specifically to the Plain of Konya and neighboring areas.

Such a view harmonizes with the more general language/farming dispersal model that may be used to explain the distribution of the languages of several of the "spread zone" language families of the world (Renfrew 2000a).

The second line of argument for an Anatolian origin, formulated by historical linguists, notably Gamkrelidze and Ivanov (1995) and Dolgopolsky (1987; 1993), considering the presence of Semitic loan words in early Proto-Indo-European, suggests an Anatolian location for *PIE on account of the adjacency of Anatolia to the regions where *Proto-Semitic is first reliably attested.

The third line of argument, again formulated by a number of historical linguists, starting with Sturtevant (see Sturtevant 1962), leads on purely linguistic grounds to the view that the Anatolian languages may be distinguished from other Indo-European languages. The first division of the original and earliest Proto-Indo-European language, designated in this view "Indo-Hittite," would be between the Anatolian languages and the rest, the basis for Sturtevant's Indo-Hittite hypothesis. According to the simplest version of this hypothesis such a view could well make Anatolia the original homeland, out of which the other *PIE languages would emerge by some migrationary process or processes. But it would also be possible, while accepting Sturtevant's hypothesis, to postulate some different and external homeland, located somewhere external to Anatolia, from which both *Proto-Anatolian and the rest of *PIE would emerge, the former reaching Anatolia through some migrationary process. That is indeed the view of some participants at our recent meeting, who accept the Indo-Hittite hypothesis without situating the original homeland within Anatolia.

As we shall see, other linguists including Adrados (1982; 1992; see Meid 1975) have emphasized the linguistic significance of this "first-split" theory by proposing a "space-time" model. The matter has been placed on a transparently quantitative basis by Ringe and Warnow (Warnow 1997; Ringe 1999). This quantitative approach does not in any sense define the location of the original —pre-split—homeland: it simply suggests linguistic relationships. But the dual nature of the split emphasizes that there are two simple alternatives for the homeland: Anatolia or elsewhere.

It is of significance that a number of scholars have recently come to look at the question with a fresh eye, and Finkelberg (1997) has stressed that the recognition of the languages spoken in west Anatolia in the Classical period, the Hellenistic period and the time of the Roman empire, as "Anatolian" (in the technical sense implying belonging to the Indo-European family and linguistically related to Hittite) has considerable implications for the traditional view of supposedly Indo-European life and society and religion. For the religious practices in west Anatolia in the classical period scarcely conform with the traditional view of Indo-European society with its conventional emphasis on male domination and mounted warrior nomads.

Moreover Finkelberg (1997) and Adrados (1992) have rightly emphasized the significance of the circumstance that the Anatolian languages were discovered, through the decipherment of the Boğazköy archives, long after the field of Indo-European studies was developed, and indeed after the development of a near consensus on the location of a Proto-Indo-European "homeland" in the steppes north of the Black Sea. With the recognition that Hittite and its relatives were indeed members of the Indo-European family came the perhaps too ready assimilation of the new data to the traditional and preexisting explanatory model (e.g., Puhvel 1994). This unquestioning assimilation, without any more analytical reexamination of the model itself, has distorted the whole field of Indo-European studies. But it is highly doubtful whether the Ukrainian homeland theory (the SCGM view) would ever have been formulated if it had already been understood that the early languages of Anatolia could also be classified as Indo-European. Only recently has the position been reassessed without those traditional preconceptions, which arose purely from the sequence of discovery, and from the circumstance that Sanskrit was accessible to western scholars much earlier than was Hittite.

Time Depth

An objection frequently raised to the language/farming dispersal hypothesis for the spread of Proto-Indo-European speech from Anatolia to Europe is the early date for that farming dispersal process. For whether or not that farming dispersal from Anatolia to Europe was associated with Proto-Indo-European speech, it *is* securely dated. The chronology for the early phase of farming dispersal has been established through the radiocarbon technique, to c. 7000 B.C. Various linguists have argued that so early a date cannot be contemplated for *PIE. But the arguments for such a view are not clear, as Dixon (1998) has robustly argued. At a symposium on "Time Depth in Historical Linguistics," held at the McDonald Institute for Archaeological Research in September 1999 (Renfrew, McMahon, and Trask 2000), there was widespread agreement that the alleged "threshold" for the age of language families, sometimes set around 5000 years ago, cannot be sustained through reasoned argument. There is no a priori method which establishes a specific threshold of time depth beyond which no traces of an early language may survive. Much earlier dates, it was argued, are possible, and Proto-Afroasiatic, for instance, is often in practice dated very much earlier. This does not in itself justify or sustain an early date for *PIE, but it does counter the argument that an early date should be excluded on a priori grounds.

In my paper to the aforementioned symposium (Renfrew 2000b), I argued that it seems reasonable to request any scholar who rejects the reasoning offered here, associating *PIE with the early spread of farming from Anatolia, on the grounds that it is all "too early," to set out clearly the rationale of the alternative (and later) chronology proposed. Reference to very general perceptions of rate of language change and of time-depth in languages families will not do, since many of these are circular, being themselves based upon a priori assumptions about the dating of *PIE. The intention was not to be polemical but to ask that any arguments as to chronology be set out clearly and explicitly, without reference to alleged "consensus" but rather with a transparent framework of reasoning. In any event there was at the symposium no challenge to an early date for *PIE. The point here is not to suggest that this itself substantiates an early date: that is a matter to be argued. It is rather that the proposal of an early date does not in itself invalidate the argument being developed.

The American linguist Terrence Kaufman, addressing this general problem (Kaufman and Golla 2000: 47), has a carefully formulated expression in which he suggests the chronological limits of the comparative approach:

> The possibility of establishing a genetic grouping requires (a) the availability of relevant data from the languages being compared, which in turn usually requires (b) that the relationship is no older than 8000–10,000 years before the earliest date at which the languages are documented.

Following this rule of thumb, and dating the earliest Hittite records to c. 1400 B.C., with the earliest Mycenaean Greek only a little later, would give an earliest date for *PIE between c. 11,400 and c. 9,400 B.C. No argument is offered here for suggesting so early a date. But the point is that there are indeed arguments, mentioned above, for postulating a date around 7000 B.C. Kaufman's position would suggest that such a date falls well within what is reasonable in terms of current understandings of time depth in historical linguistics. As emphasized above, that does not in any way substantiate the 7000 B.C. date or the language/farming dispersal hypothesis. It does, however, tend to invalidate criticisms of the hypothesis that are leveled on purely chronological grounds.

Proto-Indo-European after Anatolia

The proposed location of the original Proto-Indo-European homeland in Anatolia brings with it a number of implications and problems that cannot be ignored. The most evident of these is indeed the time depth involved. As discussed in the last section, there is no general principle that excludes an age as great as 9,000 years for Proto-Indo-European (or Proto-Indo-Hittite). But such an early date does automatically raise the question as to what was going on, in linguistic terms, in the intervening millennia between that time and the earliest well-documented Indo-European languages some five and a half millennia later.

However what at first sight may seem a difficulty soon turns out to be an opportunity, allowing one to address a number of problems concerning the relationships between the different Indo-European languages. In a recent paper (Renfrew 1999a) I have suggested, following the insights of Meid (1975) and Adrados (1992), that it is appropriate to think in terms of several stages in the development of Proto-Indo-European. This is a topic also treated illuminatingly by Lehmann (1995), although he might not favor the divisions or the terminology I propose.

In figure 1 I have taken the rooted evolutionary tree for Indo-European obtained by Ringe and Warnow from their quantitative cladistic study (Warnow 1997: 6588) and have added "boxes" and phase numbers relating to the divisions I have proposed (Renfrew 2000b, fig. 3). The structure of the tree, therefore, is entirely due to Ringe and Warnow, while the additions are my own.

The scenario I would propose is that the earliest form of Proto-Indo-European (which one might also [following Diakonov 1990] call Pre-Proto-Indo-European, or following Sturtevant [1962] "Indo-Hittite") was spoken in Anatolia c. 7000 B.C. It was the development of farming in south-central Anatolia and its subsequent dispersal that produced the "punctuation" (see Dixon 1998) that also accounts for the early spread of *PIE from Anatolia to Europe. This was, at least in part, accompanied by a process of what Ammerman and Cavalli-Sforza (1973) would term "demic diffusion" resulting in a "wave of advance" that carried the new economy and the accompanying *PIE language across the Aegean and into Greece and the Balkans. It should be noted that the new farming economy was partly derived from the Levant. There is evidence that some domesticates, specifically einkorn (Heun et al. 1997), were domesticated in Anatolia, and it is not necessary to assume that there was an influx of new population into Anatolia at the outset of the Neolithic. (It is possible that Proto-Afroasiatic was spoken in the Levant at that time [see Renfrew 1991; Diakonoff 1998]).

There is controversy over the extent to which the spread of farming to Europe was effected by a demic diffusion process (Zvelebil 1986), and it is indeed likely that this mechanism carried farming only as far as the Hungarian Plain or perhaps the westernmost extent of the Linearbandkeramik culture—i.e., into north Germany, Holland, and east-central France. Beyond that area, as Zvelebil (1995) proposes, the

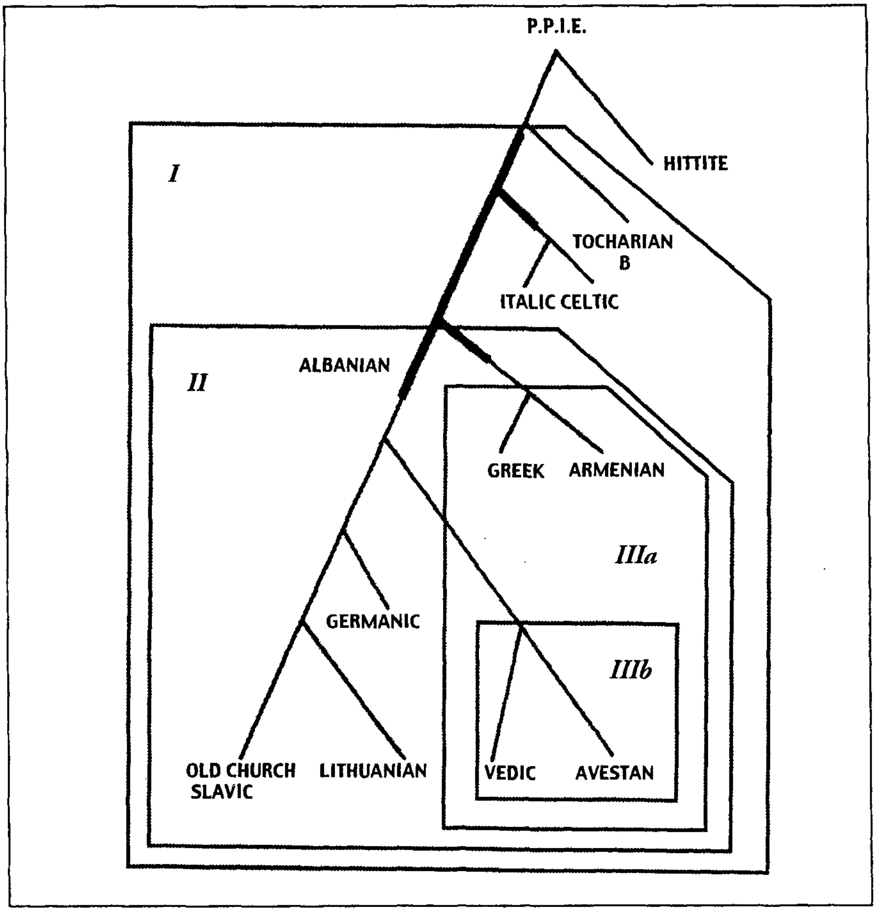

Fig. 1. The rooted evolutionary tree for Indo-European proposed by Ringe and Warnow (Warnow 1997) with the phase configurations suggested here (see Renfrew 1999 and 2000b)

adoption of PIE speech would have been much slower, by a process of contact-induced language change, so that the westernmost areas, and especially Spain, would continue to speak the pre-Indo-European language or languages of the local Mesolithic. In north Spain at least that may well have been an ancestor of Basque.

Some of the languages of what one might describe as the peripheral areas of Europe would be descended from the proto-language or languages disseminated in this PIE I phase, including the Celtic languages to the west and Tocharian to the east. Most of these languages developed, from a PIE base, in the regions where they are attested today.

The second suggestion here is that there developed in the Balkans and adjacent areas (in the region which Marija Gimbutas used to term "Old Europe") during the later Neolithic period, a convergence area or *Sprachbund*, with many shared innovations, including polythematic verbs and stems. Indeed, rather than using the term *convergence* for this process, I prefer to speak of "advergence," implying that the interacting languages in question (whose shared innovations determine the extent of the *Sprachbund*) were indeed related at the outset, as a result of the initial spread of PIE. Subsequently, however, over the space of a thousand years or more, represented by the early Neolithic, divergence processes operated in the usual way, creating dialects and then perhaps separate languages. With the emergence in this area of the cultural interactions of the late Neolithic and Copper Age, it is suggested that advergence processes operating in central Europe and the Balkans brought these languages more closely together. The area of particularly strong advergence interactions is seen in figure 2. It should be noted that advergence processes of this kind may in part be responsible for the development of subfamilies within the IE language family. It is, of course, divergence that brings about the development of separate languages with the passage of time, but it is advergence that unites these together in recognizable subfamilies (Baltic, Italic, Germanic, Celtic, etc.). The peripheral languages did not adopt the innovations associated in the Balkans with phase II (see Crossland 1967: 866). As Polomé (1987: 235) remarks, Proto-Germanic left the Indo-European community before the "preterital" development into an imperfect and an aorist, before the formation of the subjunctive and the future, but after the optative had been constituted into a separate modal form.

It was the fragmentation of the Balkan advergence area around 3000 B.C. that led to the development of separate languages in the Balkans where there had previously been only dialects. It is not suggested that this process required any population movements. It was simply that with the end of the European Chalcolithic the strong cultural interactions marking the "Old Europe" episode discussed by Gimbutas came to an end, and the various sub-regions tended to go their own separate ways.

One of the most difficult problems arising in the study of the Indo-European family is to explain the resemblances between Greek and Indo-Iranian (as represented by Sanskrit), including the sigmatic aorist, which are shared by few other languages, and features noted by Drinka (1995: 4) such as a similar theme vowel subjunctive and reduplication as the obligatory marker of the perfect tense. At this point Armenian (Clackson 1994) and possibly Phrygian (Diakonoff and Neroznak 1985) also come into the picture, sharing some of the linguistic features seen also in Greek and Sanskrit. Here the solution seems to be a relatively late separation between Greek (and Armenian) and Indo-Iranian. The latter would have formed the eastern lobe of the advergence area discussed above.

And in the case of Proto-Indo-Iranian there was indeed a further migratory process at this time that led to the severing of the links previously influential between Proto-Greek and Proto-Indo-Iranian. This process was the renewed and more intensive colonization of the steppe lands with the development there of mounted nomad pastoralism. This, however, was a process that led to the introduction of domesticated plants and animals, or at least to their more intensive exploitation, eastward from Ukraine toward the Urals and ultimately beyond. It was indeed associated at an early stage with the domestication of the horse as a food resource,

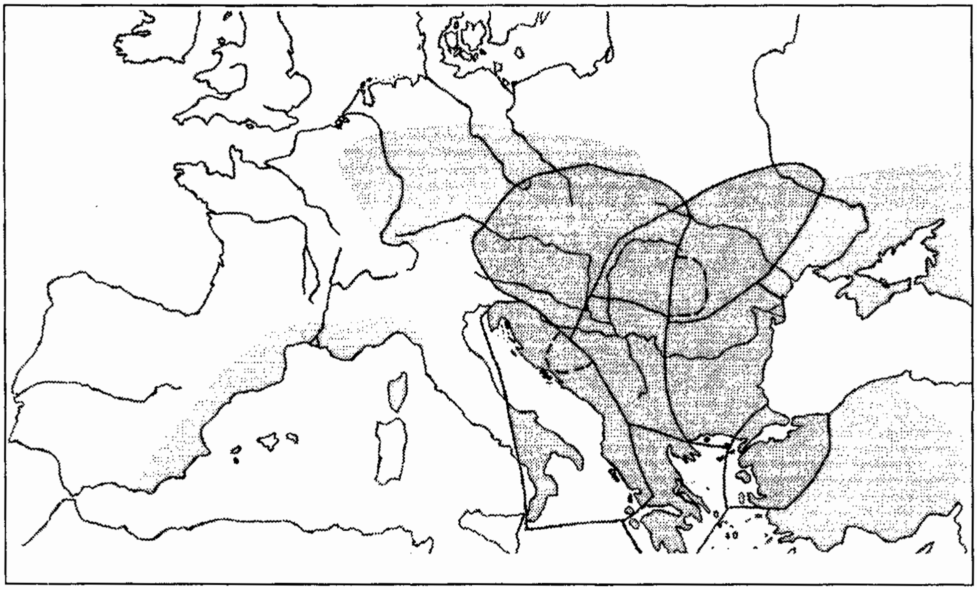

Fig. 2. The proposed linguistic advergence area of Balkan Proto-Indo-European of phase II, c. 5000 B.C. to 3000 B.C. (dark shading). To the north and west the Archaic Proto-Indo-European tradition of phase I persisted and expanded, laying the foundations for Proto-Celtic, Proto-Germanic, and Proto-Baltic. Early Steppe PIE, ancestral to Tocharian, developed to the east..

and the horse may well have been used at an early stage for herding. But it was not used for military purposes until the development of the horse-drawn chariot in the early second millennium B.C. The mounted warrior nomad horseman does not make his appearance until the end of the second millennium (Renfrew 1998b; Levine et al. 1999). It should, of course, be noted that in the earliest surviving Indo-European texts, in Hittite and Mycenaean Greek, horses are used exclusively for drawing chariots, and a comparable picture is seen in the early Vedic Sanskrit hymns.

The spread of the Indo-Iranian languages southward to Iran and then India is not a matter that can be dealt with in detail here, and indeed it presents difficult problems, as Mallory (1998) has conveniently summarized. But for our purposes, in discussing the Anatolian hypothesis, the brief outline above may be sufficient. The chronological sequence is illustrated in figure 3.

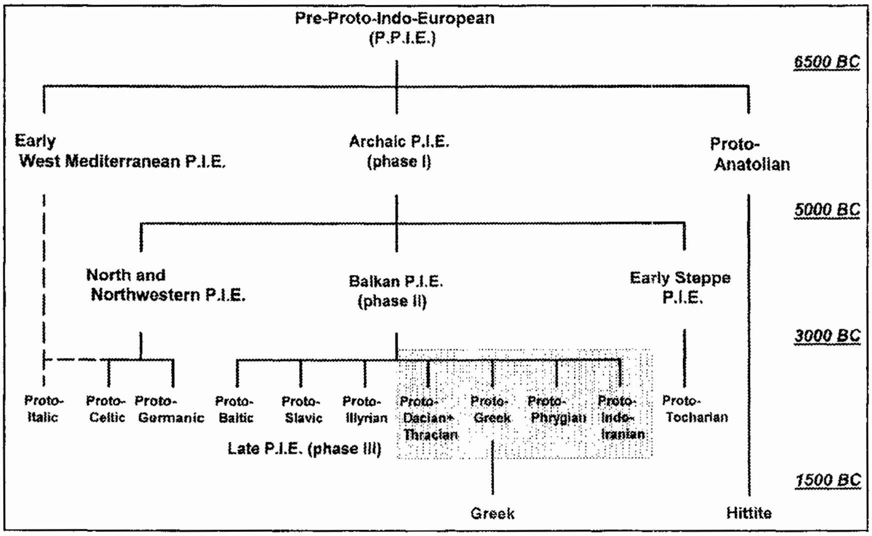

Fig. 3. The proposed temporal and spatial relationships of Proto-Indo-European (PPIE, PIE I, PIE II, PIE III, etc.)

It will be noted that this scheme differs significantly from the classical SCGM view. The transmission of Proto-Indo-European across the steppes (in phase III) is now in a west to east rather than east to west direction, and this does correspond better with current understandings of the intensification of occupation of the steppe lands (Levine et al. 1999). The mechanism of southward transmission of Proto-Indo-Iranian from these steppe lands (where Scythian was a late representative) is still unclear. The scheme differs significantly also from that of Gamkrelidze and Ivanov (1995), although they too proposed an Anatolian origin for Proto-Indo-European, albeit one located farther to the east than the Konya Plain. Their schema, however, proposes an *eastward* movement of population, out of Anatolia, proceeding around the eastern end of the Caspian Sea. It is difficult to correlate this with any known archaeological process. I would argue that the farming/language dispersal model, which brings Proto-Indo-European speech from Anatolia to the Balkans, and only then eastward to Ukraine and so (subsequently) to the steppe lands, is archaeologically more plausible.

The Horse, the Wagon and the Woolly Sheep : Linguistic Paleontology

It will be seen that the domestication of the horse plays only a minor role in this story, although it was important for the later phases on the steppe lands and the later dispersal of Indo-Iranian. The position is summarized in Renfrew (2000b) and has been discussed by Gamkrelidze (1994). It is of interest also that the horse is already found in significant quantities at early Neolithic Çatalhöyük, clearly as a food resource, as is also the case with its early appearance in high frequencies at Dereivka in Ukraine (see Levine 1990) several millennia later.

Attention has also understandably focused upon the wheel and the group of terms for wagon, axle, etc., which have a clearly identified time of origin in the archaeological record, where in Europe there are indications from the early fourth millennium B.C. It should be clearly understood however that these terms do not belong in the earliest, athematic phase in the development of Proto-Indo-European. The matter was dealt with fully by Specht (1944). As Lehmann (1993: 85) pertinently remarks:

> The study [by Specht] accordingly permits proposing chronological layers for the lexicon of the proto-language. One of Specht's most useful findings concerns his last set: terminology for wagon construction. In contrast with words for his five classes of basic items in early culture, the terms have thematic inflection. It was a simple matter to conclude that these terms were created at a late stage of the proto-language.

The Baden culture of Hungary of the fourth millennium B.C. is part of the Balkan advergence area of Proto-Indo-European phase II discussed above. Lehmann's observations may be seen to harmonize with the scenario discussed, where the athematic nouns may be seen to have been a feature of *PIE phase I, with the development of thematic nouns in phase II. The implication of Specht's observation is that the lexical innovations accompanying the invention or the introduction of the cart arose during *PIE phase II as defined here. This harmonizes with the archaeological record and with the approximate chronology proposed.

During our symposium the list of potentially diagnostic lexical items reconstructable to Proto-Indo-Hittite was discussed. Attention was very properly drawn to the term for wool, since the woolly sheep is believed to have made an appearance later than the Anatolian Neolithic, where the textiles documented archaeologically are of flax (see Barber 1991; also Renfrew 1999b). It is indeed likely that the woolly sheep was introduced into Anatolia long after the language/farming dispersal episode—here proposed as responsible for the initial dispersal of Proto-Indo-European—took place. There is thus some force to the argument that if *wool* is part of the pre-dispersal lexicon (of Proto-Indo-Hittite), then it might be more prudent to situate that dispersal *after* the introduction of the woolly sheep. At the same time, however, the sheep (no doubt of the hairy variety) is one of the commonest domesticated animals of the Anatolian Neolithic from the very earliest times (as also of the Neolithic of the Levant). The fleece of the hairy sheep would have been used for a variety of purposes, including (it may be surmised) rugs and clothing. Indeed, the hair of those sheep would also have had a number of uses during the Neolithic period. Nor is it excluded that the fibers from early Neolithic sheep were utilized to produce felt (see Barber 1975: 295). There undoubtedly was a word for *the fiber from the sheep* and for *fleece*, and it may well originally have been this meaning that had the lexical form **Hwln-* in Proto-Indo-Hittite and which, with the advent of the woolly sheep, came also to take on the meaning of *wool* (meaning the fiber specifically from the woolly sheep) by a rather modest semantic expansion. If this argument be accepted as a possibility, the potential problem for the chronology proposed here evaporates.

Indo-European scholars have sometimes laid weight on lexical arguments, depending upon the supposed appearance in the archaeological record of particular cultural items. Such an approach by means of linguistic paleontology certainly has force when cautiously and consistently applied. But it has the disadvantage of sometimes applying too much emphasis upon individual items taken in isolation. For instance, the considerable emphasis upon the horse in some later rituals among cultures speaking Indo-European languages has sometimes been projected backward to suggest some generalized *PIE belief system. Such arguments are not in themselves necessarily valid. And as we have seen above, the chronological arguments have to be considered with care.

The Relevance of Molecular Genetics

The discipline of molecular genetics is at the present time making significant contributions to our understanding of world population prehistory (Cavalli-Sforza, Menozzi, and Piazza 1994; Renfrew and Boyle 2000). There is, of course, no necessary correlation between specific genes and specific languages: the language spoken in a particular area may change by processes of language replacement. But it is nonetheless the case that a full understanding of population history must give insights also into linguistic history.

For the Indo-European case, the first hints of a possible relevance were offered by the map of the rhesus negative blood group (Mourant 1974; Sykes and Renfrew 2000: fig. 2.1), which shows a particularly high concentration in the Basque region of northern Spain. The hypothesis was proposed that Basque (the only surviving non-Indo-European language of Western Europe) and the rhesus negative frequency were both features of a relict population that had become isolated by the advent of Indo-European speech (and by implication by the accompanying genes of an incoming population).

The data base was greatly enriched by the study (Menozzi et al. 1978) of classical gene frequencies from a number of locations in Europe, followed by analysis into principal components and the representation of the variation accompanying each principal component in map form. The maps for the first three principal components are seen in figure 4.

Menozzi and his colleagues proceeded to offer interpretations of these maps. The first, representing 28 percent of the variability, was seen by them as reflecting the spread of farming from Anatolia to Europe by the process of demic diffusion. The second, representing 22 percent of the variability, proved difficult to explain, as will be further discussed below. The third, representing 10 percent of the variability, was explained by them as representing the "kurgan expansion" from Ukraine to Europe, proposed by Gimbutas (1973), and hence the dispersal of PIE.

In my earlier discussion of the Indo-European question (Renfrew 1987) I did not consider these genetic data, since they offered difficulties of interpretation—most notably the circumstance that a specific principal component does not represent the product of a single process or episode but rather the aggregate, or, one might say, the palimpsest of all the processes and episodes that have taken place over time to produce the patternings seen today. Subsequently, I accepted the proposal that PC1 represents the demic diffusion process associated with the spread of farming, and I further suggested that this patterning also reflected the spread of Proto-Indo-European speech. I suggested moreover that PC3 reflected the aggregate of east-west (and indeed west-east) diffusion processes over time, including those associated with Sarmatians, Scythians, Huns, Slavs, Mongols, and Turks (to name but a few), and that this patterning should not be seen as a strong support for the Gimbutas kurgan theory (essentially much the same as the SCGM view), which I believe finds little support in the archaeological record.

The interpretive position has been changed radically, however, through the application of mitochondrial DNA analysis (Richards et al. 1996; Sykes 1999; Renfrew 2000c). This method offers the possibility of dating the expansion process for each haplogroup in turn using internal molecular genetic arguments, without necessary reference to the local archaeological record. It leads to the interesting conclusion that the greater part of the variability seen in figure 4(a) is the result of migration processes (i.e., demic diffusion) taking place already in the Upper Paleolithic period. Indeed Sykes (1999) has offered the following table which quantifies the Paleolithic contributions and those of the Neolithic (seen mainly in Haplogroup J).

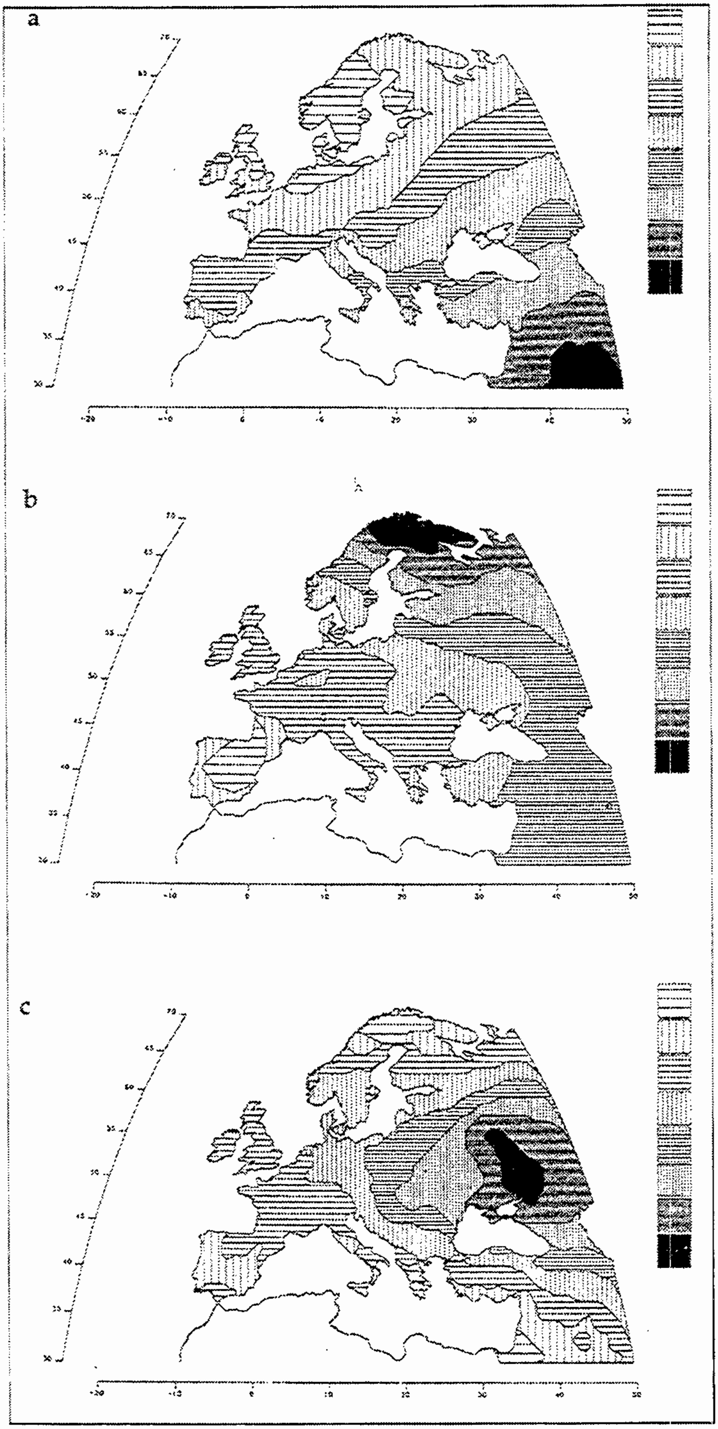

Fig. 4. Synthetic maps of Europe and western Asia obtained by Menozzi et al. (1978) using (a) the first, (b) the second, and (c) the third principal components of the variations obtained by the analysis of the gene frequencies of ninety-five genes, using classical genetic markers.

Table 1.
The three main waves of the colonization of Europe,
as inferred from mitochondrial DNA analysis,
indicating the percentage contribution to the modern gene pool
contributed by each (after Sykes 1999)

Component	Dates (BP)	Main Associated Clusters	Contribution to Modern Gene Pool
Neanderthal	300 000	unclassifed	0%
Early Upper Palaeolithic	50 000	U5	10%
Late Upper Paleolithic	11 000–14 000	H, V, I, W, T, K	70%
Neolithic	8500	J (+ more of H, T, K?)	20%

In the light of these proposals, which are contested by Cavalli-Sforza and his colleagues, the patterning seen in PC 1 would mainly be of Upper Paleolithic origin, although the demic diffusion associated with the coming of the farming economy would still be represented in the 20 percent of variability contributed by Haplogroup J. The second principal component may well be associated with the expansion of population from the Iberian (and Ukrainian) refugia following the ending some 15,000 years ago of the cold period associated with the Late Glacial Maximum. There is important molecular genetic support for this process (Torroni et al. 1998).

The third principal component (figure 4 [c]) may well prove also to have a background that is largely Upper Paleolithic in origin, representing migration into central and western Europe westward along the north of the Black Sea. It will also reflect what record there may remain of the population movements noted earlier. Supporters of the SCGM view may reasonably hope to see in it also some trace of kurgan migrations, although I am personally skeptical of these.

At the moment I think one may draw two conclusions from the available molecular genetic evidence for Europe, in relation to the Indo-European question. In the first place there is the strong possibility that the main determinant of the genetic composition of the human population of Europe today is the population history of Europe in the Upper Paleolithic period. The second is that, while demographic processes associated with the dispersal of the Proto-Indo-European language may still be reflected in the molecular genetic record, they are likely to be a relatively minor component. It will need further analysis and the application of subtle and sensitive arguments in order reliably to reconstruct and date such processes. Haplogroup J, as suggested by Sykes and Richards, may well represent the gene flow accompanying the farming dispersal process and be a marker also for the spread of the Proto-Indo-European language. But that is no more than a possibility at present. In the long term, molecular genetics may be helpful in contributing to the resolution of a number of problems in historical linguistics, but the point has not yet been reached when it might offer any firm resolution of the Indo-European problem.

The molecular genetic picture as we see it today certainly does not contradict the scenario proposed here, so long as one takes into account Zvelebil's proposal (1986; 1995) that demic diffusion is only one mechanism underlying the language/farming dispersal process and that contact-induced language change must also be considered.

Early Anatolia and the Aegean

The linguistic history of the Aegean is a complicated one, more richly documented, through the circumstance of Bronze Age literacy, than that of any other area of Europe. The Mycenaean Linear B documents of Knossos in Crete and of Mainland Greece are now recognized as being written in an early (Mycenaean) form of Greek (Ventris and Chadwick 1973). In Crete there is the Linear A script and also the hieroglyphic scripts that have not been satisfactorily deciphered and that undoubtedly reflect the language or languages of Minoan Crete. In addition, attention has frequently been drawn to the existence of a "stratum" of place names that may be recognized as non-Greek and in some cases as non-Indo-European (Haley and Blegen 1928). At the same time there is a considerable vocabulary of lexical items that may also be recognized as non-Greek and in some cases as non-Indo-European (Hester 1965; Morpurgo Davies 1986).

The assumption has frequently been made that because the place names and the vocabulary items are non-Greek they are, therefore, pre-Greek. That is to say, they would go back to a period and a linguistic stratum prior to the "coming" of the Greeks to Greece. However, the possibility has been raised (e.g., Chadwick 1963; Renfrew 1964, 1973) that the Greek language developed in Greece following an earlier arrival there of Proto-Indo-European speakers. One version of that theory has, of course, been outlined above, where the arrival of *PIE with the first farmers is posited, and then the development of Proto-Greek within Greece. During *PIE phase II Greece formed part of the Balkan advergence area. The ties broke and a greater degree of isolation followed after c. 3000 B.C., but not before the affinities between Proto-Greek and Proto-Indo-Iranian developed, as mentioned above.

I have proposed (Renfrew 1998a) that a major component of the non-Greek vocabulary seen in the Greek language actually derives from the Minoan language of Crete, as seen in the Liner A tablets. A comparison of the lexical items listed by Hester (1965, section D) as "Aegean" (i.e., non-Greek) with the archaeological record suggests that they would be very much at home in the palace civilization of Minoan Crete but not at all so in the much earlier cultures of Neolithic Greece, to which they might have to be assigned if the classification as "pre-Greek" were taken seriously.

This is not the place for a careful discussion of this view. But its acceptance would remove the need for a pre-Greek (and non-Indo-European) linguistic stratum in Greece surviving on to take its place in the language of classical Greece. It is, of course, possible that the non-Indo-European language of Upper Paleolithic and Mesolithic Greece did indeed leave surviving traces. But my suggestion is that the greater part of the vocabulary in question reflects an adstratum of borrowings from Minoan Crete.

The circumstance that Crete was first settled at the beginning of the Neolithic period, probably from southwest Anatolia, raises the question as to whether the first language spoken in Crete might have been "Anatolian" or at least "Proto-Anatolian," in the linguistic sense. It is not, of course, certain that the Minoan language of the Linear A tablets was the lineal descendent of the language of Neolithic Crete. But that is at least a strong possibility, and in a sense the simplest one. It may well be that there was a three-way split of the earliest PIE (i.e., Pre-Proto-Indo-European or Indo-Hittite) into: (1) Proto-Anatolian and (2) Narrow Proto-Indo-European (such being the Indo-Hittite hypothesis) and also (3) Proto-Minoan. In that view the Minoan language would be an Indo-European language, but not in the sense in which that term is usually understood. It would stand in much the same relationship to the bulk of the Indo-European language family as does Hittite (along with the other Anatolian languages), the product of divergence over a period of at least five millennia, following a split occurring around 7000 B.C. That is all very hypothetical, but it is at least a possibility worth mentioning.

This discussion may be worthwhile because it removes the necessity for introducing further hypothetical language entities, such as a distinct pre-Greek language in Neolithic or Bronze Age Greece, or some hypothetical "Pelasgian" language (see Georgiev 1973), unless that be interpreted simply as the Proto-Indo-European speech that came to Greece with the first farmers and gradually developed into Proto-Greek.

The simplicity of this picture is supported by the work of Carruba (1995), who notes that in western Anatolia, west of what he terms the "Anatolian diagonal," which runs from the Bosporus to the Gulf of Alexandretta, the recorded place names are exclusively Indo-European, many of them in the Anatolian language, Luwian (figure 5). (Carruba borrows the term *Anatolian diagonal* from Mellaart's map of the late Chalcolithic period [Mellaart 1975: map 7] and draws attention to it in emphasizing the strength of the Luwian linguistic presence in west Anatolia):

> La toponomastica antica dell'Anatolia mostra al disotto della "diagonale anatolica" (una linea fra gli Stretti e il golfo de Alessandretta) una luvizzazione così intensa e profonda da rendere difficilissimo, se non impossibile, un rilevamento del sostrato "preluvio." (Carruba 1995: 18)

This harmonizes well with the general picture presented here. For it is the key underlying proposal that the homeland of the earliest Proto-Indo-European falls

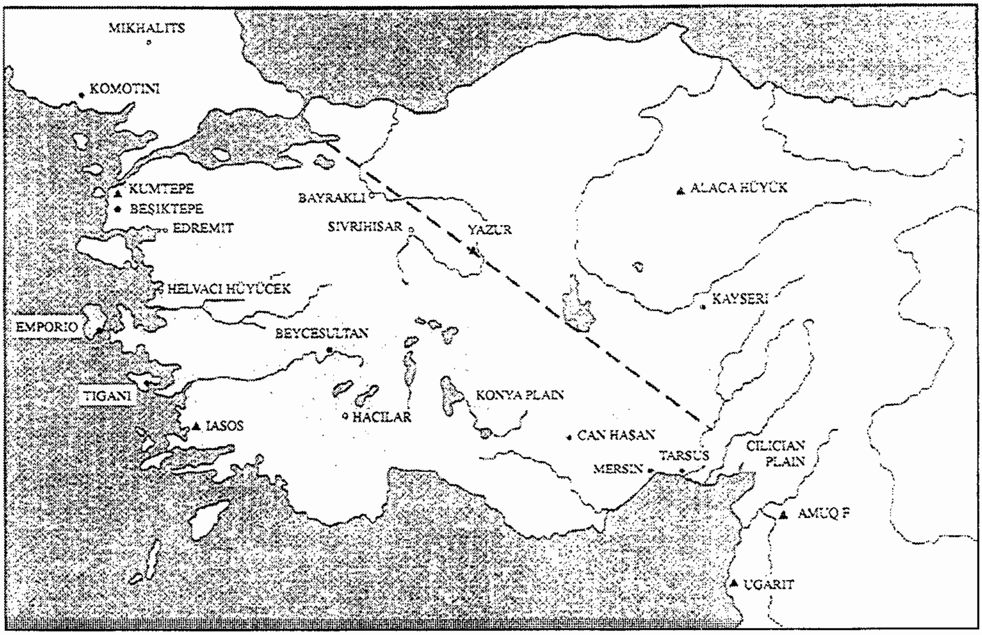

Fig. 5. The "Anatolian diagonal" (after Carruba 1995: 19), west of which a pre-Luwian substrate cannot be detected among the place names.

within the southern half of the zone below this line, and specifically in the Konya Plain and the area to east and west of it. It was here that Proto-Indo-European would have evolved into Proto-Anatolian and from this again that the Anatolian languages of the Bronze Age (including Hittite and Luwian) and ultimately those of the classical period emerged. These relationships are summarized in Figure 6.

It should be noted that Phrygian, although Indo-European, is not classified linguistically as an Anatolian language. It shows affinities with Greek and probably Thracian (Diakonoff and Neroznak 1985) and perhaps with Armenian also (Diakonoff 1984). It seems likely that the presence of Phrygian in north-central Anatolia is the result of some localized population event in the northern Aegean and northwest Anatolia toward the end of the second millennium B.C. It may be that the same applies to the present location of the speakers of the Armenian language, although this is a yet more difficult problem.

The developing knowledge of the North Caucasian languages has permitted recognition of a complex language family with potentially very wide relationships (Starostin 1991). Among these it is now recognized that Hattic may be classed with the West Caucasian branch of the North Caucasian language family (Ivanov 1985;

Diakonoff 1990). Thus there is the possibility that Hattic is not a language indigenous to central Anatolia but one which may instead have come to central Anatolia from the northeast, from its homeland in the Caucasus. One may consider the possibility that Hittite may have been at home in south-central Anatolia, where there is evidence from its use at Kanesh (Hittite: *Nesa*) at a date earlier than it is attested at Boğazköy in the north. The local chronological priority of Hattic over Hittite, which is indeed evident specifically in the tablets found at Boğazköy, can no longer be assumed to be of general applicability. Hattic is now recognized as a Caucasian language and may itself be the intruder in central Anatolia.

When we look at what is known of the linguistic history of central and western Anatolia, we see very little that is not Indo-European. The language of the stele of Lemnos remains a problem, and a suggestion of a North Caucasian relationship has been claimed, although it has also been seen as the product of a late Etruscan colonization from the west. Naturally the presence of Greek speech in Western Anatolia in classical times is the result of a local colonization process in the late Bronze Age (Mountjoy 1998).The geography of western Anatolia in the late Bronze Age is increasingly well understood (Hawkins 1998), and it now seems that all the well-researched languages of western Anatolia in classical times, apart from Greek, are to be classed as Anatolian (including Lydian and Lycian). In the light of recent researches this is now true for the Carian language also.

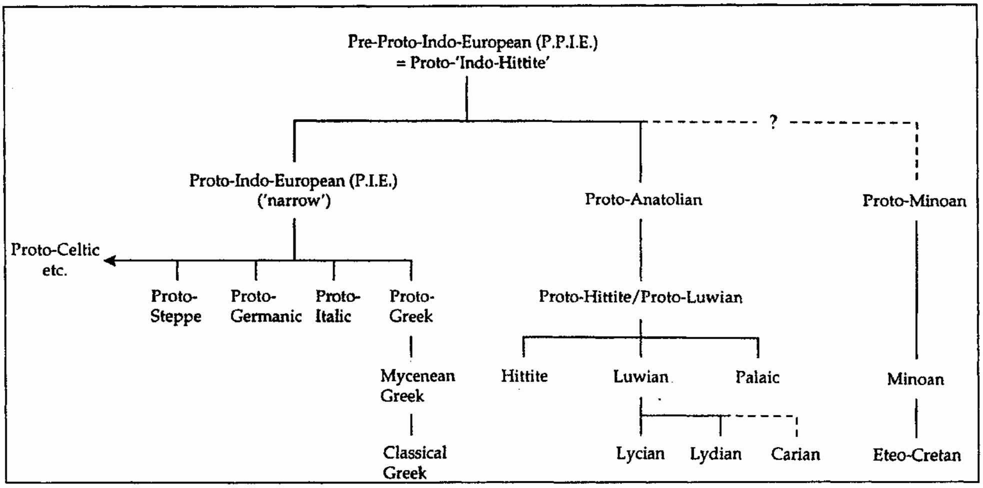

Fig. 6. Hypothetical ancestral relationships between the languages of the Aegean and Anatolia in the Bronze Age. The dotted line represents the questionable hypothetical relationship between the proposed Proto-Minoan language and the early common ancestor of the Proto-Anatolian and of the ("narrow") Proto-Indo-European languages.

It may be concluded, then, that there is no evidence for any non-Indo-European linguistic presence in the Aegean or west Anatolia (other than possibly Lemnian) or in south-central Anatolia in the Bronze Age or in classical times. It is possible that there has been linguistic continuity in these areas, albeit with minor local shifts, since the first Proto-Indo-European of c. 7000 B.C.

Indo-European Societies and Indo-European Myths

Perhaps the greatest obstacle to the acceptance of the Anatolian origin of the earliest Proto-Indo-European language is the myth—in the contemporary sense—which has grown up over the past century and a half concerning Indo-European mythology, society, and belief systems. It springs in part from the notion that there was a *single* Proto-Indo-European society, at some notional (but often ill-defined) place, whose environment, material culture, and social institutions would be the counterpart of the single reconstructed Proto-Indo-European language, proposed by the application of the comparative method. From this homeland there would have radiated, in all directions and at much the same time, the tribes purportedly ancestral to the speakers of the various subfamilies of the Indo-European family. If there were shared features, for instance between Greek and Sanskrit, this would be explained by the circumstance that the Proto-Greek speakers and the Proto-Indo-Iranian speakers would have lived in proximity in the postulated homeland (although just *where* they lived in proximity would not be vouchsafed) for some time before the dispersal. In the writings of Dumézil, for instance, we have erudite speculation about the social system of the Proto-Indo-Europeans in this mythical land, although he never offered any concrete suggestions as to just where that land might have been, or when the "dispersal" took place.

It would, on the contrary, follow from the scenario outlined above, and the very great time depth within which it operates, that there was *no single Proto-Indo-European homeland.* The proposal is that south-central Anatolia was only the first of a succession of such regions where Proto-Indo-European language and society, or rather languages and societies, developed. Also of central relevance to the overall picture is the "Old Europe" advergence area of the late Copper Age, where many of the linguistic features often conceived as typically *PIE developed. Its society would have differed in many ways from that of Neolithic Anatolia. Again, the societies, or at least the languages, of Bronze Age northern and northwestern Europe, where the Germanic and Celtic languages were developing *in situ,* may very properly be considered as Proto-Indo-European, albeit five millennia later than the first Proto-Indo-European of Anatolia. And, of course, it was not until the European Iron Age that the stereotype image of the mounted warrior, so popular with the SCGM view, became a possibility.

The key to breaking down these stereotypes is to accept that in a strict sense the terms *Indo-European* and *Proto-Indo-European* are meaningful only when applied specifically to languages, not to social institutions or belief systems. And as I have tried to indicate above, they can be used with clarity only when some geographical and chronological precision is adduced. It has to be insisted that there are no universal generalizations to be made about the religions, the social systems, or the myths of the various communities who, over a wide range of time and space, have spoken languages that may accurately be classified as Indo-European.

Margalit Finkelberg (1997: 15) has lucidly criticized the conventional oppositions between patriarchal vs. matriarchal, warlike vs. peaceful (and agricultural), and nomadic vs. sedentary as markers of Indo-European vs. non-Indo-European societies. It should be clearly realized that the only subfamily to which the term *nomadic* could possibly be applied with reason would be the Indo-Iranian subfamily of the steppes. There are simply no other plausible candidates. Nor does there seem to have been anything very nomadic about the Indo-Aryan societies of the Indian subcontinent either. As Finkelberg implies, the dichotomy between patriarchal vs. matriarchal goes back to the outmoded ideas of Bachofen. And, of course, Bachofen's paradigm case for a matriarchal society was the very world of west Anatolia in classical times (Lycia and Caria) whose languages in the light of recent scholarship are now regarded as Indo-European! Great is Artemis of the Ephesians! It was not realized in the nineteenth and early twentieth centuries that the languages of these societies were all, in fact, Anatolian languages and thus members of the Indo-European family.

It is this sort of misunderstanding that leads Lehmann (1993), so reliable a guide in linguistic matters, into what one may describe as a category error, when discussing the possibilities of early Indo-European religion:

> Any examination of the wall paintings at Çatal hüyük leaves little question that the culture of the Indo-European speakers as indicated in their early texts was totally different from that of the peoples at Çatal hüyük. There is no reflection in any site associated with the Indo-Europeans of "skeletons, often with their skulls painted and decorated . . . buried under the sleeping-platforms in houses." Similarly the shrines dedicated to the goddess are totally out of keeping with Indo-European religion. (Lehmann 1993: 264)

The comparison fails because Lehmann is juxtaposing what we see in the Hittite and Mycenaean texts around 1300 B.C. with archaeological findings in south-central Anatolia around 7000 B.C. Not surprisingly they are very different from each other, just as the citizens of Ephesus had different temples and religious observances from those of the Scythians at much the same time, as described by Herodotus. Or again, Mycenaean religion in 1300 B.C. was very different from that of the classical Greeks only 800 years later: yet both societies spoke effectively the same language, and not merely languages within the same family of languages.

To make these observations is not, however, to imply that the considerable scholarship lavished upon the study of "Indo-European" social structures and belief systems has been misplaced. The point is merely that the term *Indo-European* simply means that the people concerned spoke languages within the same language family, a family with a vast extent in space (as all will agree) and (as is argued here) a very considerable depth in time. It may well be possible to study those further features that are indeed common to particular groups of cultures, well defined in space and time, and to use the lexicon or lexicons, insofar as they can be reconstructed *in relation to those cultures* in an illuminating way. Put concisely the simple point is that the term *Indo-European,* without further qualification, refers to too broad a range and too great a variety of contexts for it to have very much meaning—other than in well-defined linguistic terms.

Continuities in Anatolian Prehistory: The Origins of the Hittite State

There seems little doubt that sedentism preceded full agriculture in southeast Turkey (Özdoğan 1995a; 1995b). The domestication of einkorn in southern Anatolia (Heun et al. 1997) has already been mentioned. The Anatolian Upper Paleolithic and Mesolithic is not yet well understood, but there is at present no reason to suppose that early farming in Anatolia was itself the product of some demic diffusion process from some earlier agricultural center developing in the Pre-Pottery Neolithic A culture of the Levant, as has sometimes been claimed. Incipient domestication seems to have been a feature of several regions neighboring the "hilly flanks of the Fertile Crescent." Southern Anatolia is within the natural distribution of several of the wild progenitors of the plant and animal species that became the staples of early farming, and it is likely that the ancestors of the first farmers of Anatolia were themselves residents of much the same area, rather than incomers participating in a demic diffusion process originating elsewhere.

It is, of course, an interesting speculation to inquire into these putative ancestors of the speakers of what we have suggested was the earliest Proto-Indo-European speech. Certainly one could imagine three lines of investigation into the ancestry of the early Neolithic population of south-central Anatolia. The first would involve the archaeology—the culture sequence leading from Upper Paleolithic, possibly to "Mesolithic," and so to early Neolithic, such as is currently dimly understood from researches in such nearby cave sites as Beldibi and Karaïn. The second will one day involve a more detailed study of the molecular genetics of the present populations of southern Anatolia which, despite the passage of the millennia, may still within their genes carry information about the local populations of ten thousand years ago, just as appears to be the case in Europe, as we have noted above. The third, perhaps more controversial, approach is to consider the possible linguistic ancestry of early Proto-Indo-European, whether 'Nostratic,' to use the terminology of the Russian school of macrofamilial researches (Dolgopolsky 1998; Renfrew and Nettle 1999), or 'Eürasiatic,' to follow that of Greenberg (2000). All three approaches are, however, at present speculative, and more work is required before they can be taken further.

The sequence of cultures following the early Neolithic is a little clearer. Even so, the later Neolithic and Chalcolithic culture sequence is long and not yet entirely understood. The suggestion of cultural continuity, give or take local displacements, in south-central and western Anatolia must, however, now be taken seriously. In particular one must now begin to imagine some degree of continuity from the early Proto-Indo-European postulated here for this area (with a territory including sites on the Konya Plain) through to the Proto-Anatolian language that would have followed in the late Neolithic and Chalcolithic, and then on to the Proto-Hittite and then the Hittite languages, respectively, of the earlier and later Bronze Ages.

The *karum* at Kanesh (Kültepe) gives indications of Hittite, mainly in the form of personal names preserved in the archives of the Old Assyrian traders (Veenhof 1989), from as early as 1800 B.C. The use of the term *nesili*, meaning *in Hittite* in the Boğazköy tablets, is a clear hint that the Hittite language was more at home at Nesa (i.e., Kanesh) than at Hattusas. And while Hattusas (i.e., Boğazköy) became the capital of the Hittite empire, the clear relationship between that name and the designation *hattili* (i.e., in Hattic), does suggest a special association between that site and the Hattic language in the centuries prior to the destruction of the metropolitan archive. However, as noted above, the Hattic language is classed by Ivanov (1985) with the West Caucasian branch of the North Caucasian language family. We have, of course, no way of judging the extent of distribution of the North Caucasian languages in Anatolia at that time. Hurrian and Common Hurro-Urartian are placed by Diakonoff (1990) in the Eastern Caucasian branch of the North Caucasian family (see also Wilhelm 1989). While it cannot necessarily be assumed that the Hattic language originated in the Caucasus, that may well seem probable. There is nothing to conflict here with the view that early Proto-Indo-European was indigenous to south-central Anatolia (and probably the North Caucasian languages native to northeastern Anatolia and the Caucasus). Early Proto-Indo-European (i.e., Proto-Indo-Hittite) was, of course, the presumed point of departure not only for the rest of the Indo-European language family but also for Proto-Anatolian, initially in southern and probably also in western Anatolia. Whether some form of Proto-Anatolian was spoken in the area of Hattusas before the arrival there, possibly from the Caucasus area, of the Hattic language cannot now be established. But there seems nothing surprising in the notion that Hittite came at Hattusas to replace Hattic, and that it came from the south.

Clearly, this runs counter to the assumptions of many Hittitologists (e.g., Puhvel 1994). But as we have seen, their context of thought has generally been structured by assumptions about the origins of the Indo-European language family that are rooted in the nineteenth century, long before the Hittite archives were discovered or Hittite itself was recognized as an Indo-European language. We must therefore face the clear possibility that the Hittites were an autochthonous people, with a long ancestry of several millennia in the area of Kanesh and the lands to the west. That, of course, harmonizes with Carruba's view (1995) that there is no pre-Luwian substratum in south-central or western Anatolia, Luwian being a sister language of Hittite. We must therefore also face the implication that the Hittite civilization was also a local development.

That is not, of course, to assert that state society in Anatolia, and the Hittite civilization, developed in isolation. Already from the time of Çatalhöyük and Asıklihöyük there was an extensive supply of Group 2b obsidian going from local sources in south-central Anatolia to the lands to the south. By the fifth millennium B.C. pottery with Halaf affinities believed to be at home in northern Mesopotamia is found in south-east Turkey (Campbell et al. 1999: fig. 1), and with the subsequent site of Arslantepe, also with notable southern connections, we are on the brink of urban society. There are thus grounds for seeing Hittite urbanization in some senses as a "secondary civilization," as the Old Assyrian outpost at Kanesh would serve to suggest, with its clear intimation that writing and bureaucratic archives first came to Anatolia as the product of a well organized Mesopotamian trading endeavor.

But the concept of secondary civilization is often not a very satisfying one, and one should not overlook developments during the Early Bronze Age of the third millennium B.C., as documented so notably by the rich burials at Alaca Hüyük, which indicate strong local traditions (Akurgal 1989). Nor should western Anatolia be overlooked. As Machteld Mellink (1998: 2) reminds us, writing of a treasure from the Early Bronze III period found near Alaca, "Multiple references to the jewelry idiom represented at Troy are unmistakable and help to reveal interconnections of Troy with the area which was to become the Hittite coreland."

It is to be anticipated that when the origins of the Hittite civilization at last come to be debated by archaeologists in terms of the workings of culture process and as a striking example of early state formation, there will be a debate that may in some ways come to parallel the arguments surrounding the origins in the Aegean of the Minoan and Mycenaean palace civilizations. A polarity of explanation can be seen there also between "primary" and "secondary," between innovation and diffusion, between the endogenous (local) and the exogenous (Near Eastern). Such debates can rapidly become sterile, since it is clear both that there were indeed trading contacts with existing urban societies further south and that there were also strong local traditions rooted in preexisting social and economic factors. The Hittite civilization is now ripe for systematic scrutiny of this kind. But at least there is the opportunity of conducting such analysis without the crippling prior assumption that the key event in the entire process was the advent of the Hittites, emerging like a deus ex machina, from some still unidentified homeland in the north. It is many years since Aegean archaeologists dismissed "the coming of the Greeks" as a non-event, a confusion arising from a misreading of the classical sources. It is time that the notion of the "coming of the Hittites" was similarly abandoned. Then, at last, it will be possible to see some progress in the study of Hittite society and culture and of their prehistoric antecedents.

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CRITICAL RESPONSE TO THE FIRST FOUR PAPERS

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It is an honor and a privilege to have been invited by Professor Drews to comment on papers by archaeologists of the stature of the four speakers we have so far heard at this colloquium, all highly productive scholars who in quite disparate ways have made impressive contributions to our understanding of Old World material culture. At the same time, it is a formidable challenge to respond to four papers as varied in their subject matter as these are and that in every case have profound implications for the subjects we are addressing at this scholarly meeting. As an Aegean prehistorian whose focus of research has been principally the ceramics, relative chronology, and intercultural contacts of the Bronze Age, I cannot pretend to have the requisite background to provide a credible critique of all four of these presentations. But perhaps I can stimulate a question-and-answer period by restating some of their major points that I found particularly intriguing, by drawing attention to at least some of the more obvious implications of these, and by posing a few initial questions. Rather than attempt some eloquent synthesis of the full range of topics to which the speakers have exposed us, let me simply address each of the four papers in the sequence in which they were presented.

I am not sure I can agree with Elizabeth Barber that "clothing became second in importance only to language for daily communication of human messages" from the Late Neolithic period onwards. Having just taught an undergraduate seminar on eating and drinking in classical antiquity, I cannot seem to get food and foodways out of my mind as humanity's second most important mode of communicating. But I certainly share her enthusiasm for the potential significance and relative scholarly novelty of the rich body of information inherent in ancient Old World textiles, the attendant techniques and traditions of which she has so vividly brought to our attention over the past decade.[1] As she has noted, one of the most striking facts about this corpus of material is how closely archaeological and linguistic data can be juxtaposed in efforts to trace the spread of weaving technologies. Her own training in both those fields, coupled with her expertise as a weaver, have made her an unusually eloquent and persuasive exponent of the relevance of textiles and of the vocabulary associated with their production in making progress on several long-debated questions involving Indo-European dispersals. For example, even if clothing "evidently began not for warmth but to carry social messages," Barber has reminded us that "woolly sheep, along with horses and wheels, were what made steppe nomadism a possible way of life." She takes the presence of a Hittite word for *wool* that is clearly cognate to words with the same meaning in numerous Indo-European languages—along with a relatively late date of ca. 4000 B.C. for the emergence of woolly sheep and the consequent large-scale exploitation of wool as a textile fiber—as clear evidence that "Anatolian and the other [IE] groups must not yet have parted company at roughly 4000 B.C.," a conclusion clearly at variance with Renfrew's "demic diffusion" model of Indo-European speakers migrating out of Anatolia as early as the mid-seventh millennium B.C. Need a relatively late date for the appearance of woolly sheep and the initial use of their wool in woven cloth be the determining issue in assessing the viability of Renfrew's theory for a very early southern Anatolian origin of all Indo-Hittite languages? Or could a word for "wool" somehow have come into existence long before spun wool fibers became an important component of human clothing?

Barber's two books of the early 1990s have demonstrated that the overwhelming majority of prehistoric weavers in the Old World were women. She has therefore claimed that the movement of the tools and technology associated with weaving are singularly valuable in allowing us to distinguish the movements of entire families—that is, women and men together—from the movements of looting raiders or of itinerant artisans. But she has herself drawn attention to the important place occupied by substantial numbers of captive women, presumably slaves, who were active in the Mycenaean palatial weaving industry at Pylos and are described in the Linear B documents from that site by the toponyms of their places of origin.[2] So, in theory, adult women could have been moved around independently of their families. I wonder if weaving technology might as a consequence also be able to move independently of entire families and, by extension, also of larger kinship groups?

Above all, Barber's discussions of weaving tool kits—looms and their sometimes associated loom weights, spindles and *their* associated weights that we term whorls, and other less morphologically varied items like needles and dye vats, as well as less reliably identifiable artifacts like bobbins and shuttles—provocatively juxtapose the evidence from both artifacts and language. But is her interpretation of the significance of the Greek language's double set of weaving vocabulary—an Indo-European set connected with band weaving and borrowed words for weaving wider cloth on a warp-weighted loom—the only one possible? For example, loom weights reveal that warp-weighted looms were in use on the Greek mainland as early as the mid-fifth millennium B.C. (Middle Neolithic at Franchthi Cave), but such weights disappear rather abruptly ca. 2300 B.C. at the end of the Early Helladic II period, only to be reintroduced from Crete in an altogether new form some six centuries later. Spindle whorls during the same period also change, albeit in less dramatic ways.[3] The disappearance of the warp-weighted loom is probably to be connected with a series of other dramatic changes in central and southern Greek material culture ca. 2300 B.C. that some have interpreted as representing an influx of new people.[4] Whether or not a population movement of some sort is involved, life on the mainland for several centuries after 2300 B.C. certainly appears to have been less settled or, alternatively, more nomadic than before. The evidence from architecture and regional patterns of settlement is quite clear in this regard, and, consequently, the abandonment of large looms is hardly surprising. I do not want to open up a Pandora's box by invoking large-scale population movements; I simply want to inquire what the impact on weaving terminology is likely to have been in the affected region when a long-established tradition of employing the warp-weighted loom was abruptly abandoned for half a millennium, to be re-instituted as part of a substantial package of new technologies and art styles introduced from Minoan Crete toward the end of the Middle Bronze Age. Would a double or even triple set of weaving vocabulary not be among the possible results? And with which culture or cultures should Barber's various items of "borrowed" terminology be connected?

It is certainly a remarkable circumstance of discovery that in all of Aegean prehistory the three actual looms and loom locations about which we are perhaps best informed—one from the burnt remains of Troy IIg, one from Aphrodisias in the southwestern Anatolian interior, and one from Pefkakia in coastal Thessaly on the other side of the Aegean—all date to within a century of each other ca. 2500 B.C. and are found in strata that contain substantial quantities of Anatolian west-coast pottery, including the well-known but misnamed "depas amphikypellon."[5] Contemporary weavers on Crete and in the Cyclades appear to have positioned their warp-weighted looms on the flat roofs of their pueblo-like Early Bronze Age houses. However, the pitched roofs of the Thessalian and probably also the Trojan and Aphrodisian buildings from which the evidence for those looms comes required a ground-level location for looms, which prevented their weights from being scattered all over the place when those buildings burned. Minoans and Anatolians in the Early Bronze Age had at least one other significant weaving difference that deserves mention: whereas Anatolian spindle whorls are extremely common, and at some sites such as Troy number in the thousands, the number of items identified as Minoan spindle whorls is absurdly small. A recent count by Tzachili identified only forty-eight Early Minoan whorls from all of Crete, with even fewer coming from Middle and Late Minoan levels.[6] Either Bronze Age Minoans spun in a different way altogether, or the material that they used for their spindle weights was atypical (wooden, like the spindles themselves?), and, consequently, the whorls have not survived.

Barber's identification of the morphologically distinct "hollow-topped" whorls of Anau, Troy I–III, Early Helladic III through Middle Helladic Greece, and finally Late Bronze Age northern Italy and Switzerland as combination whorl-shuttles is persuasive. I am delighted to accept these as one of several indicators of an increased degree of nomadism. But why they should be viewed as especially connected with the steppe, even when found in an Aegean or Italian environment, is not clear to me. I am skeptical of the north-to-south vector she posits for the spread of such whorls in Anatolia from a starting point on the other side of the Sea of Marmara and, by extension, ultimately somewhere on the Pontic steppe. But the problem of tracing and interpreting the diffusion of individual items of weaving technology makes me wonder how feasible it might be to assemble a diachronic series of maps of entire weaving traditions—loom weights, whorl types, ranges of fibers and weaves in use, and so forth—showing how such items shift and change within the regions of Eurasia of greatest relevance to the Indo-Hittite hypothesis. Would such maps have any real value in the face of all the unfortunate gaps in the archaeological record that are the result of poor preservation and inadequate publication? Or might the superposition of such maps over those plotting various models of Indo-European language spread perhaps reveal some illuminating patterns?

* * *

As an archaeologist, I fully share Paul Zimansky's "uneasiness" about contributing a paper at a colloquium organized around a linguistic problem. Indeed, my unease is intensified with regard to his paper in particular because of my general ignorance about matters Urartian! But fortunately it does not take any special expertise to see that his two principal themes, one archaeological and one linguistic, are right on target for a colloquium such as this, where an effort is being made to link archaeological and linguistic data, even though the time period that he is concerned with is considerably later than those at issue in our three other archaeologically oriented papers. On the archaeological end, Zimansky maintains that much of Urartian material culture appears to have been a quite suddenly devised construct intended by its elite creators to obliterate any cultural identity other than membership in the polyglot empire that this elite had cobbled together. Zimansky draws the comparison between Urartian culture and the kind of uniform material assemblage characteristic of the Incan Empire. This comparison together with his contention that the Urartian rulers promoted this assemblage by means of a systematic program of population relocations within an unusually forbidding natural environment make the rise of the Urartian state and its more than two-century-long floruit a striking example of the possible dissociation of material culture and language, especially insofar as the Armenian version of Indo-European is concerned. If he is correct in his reconstruction of how Armenian came to be spoken in that portion of eastern Anatolia that eventually became its "homeland," surely Armenian is unusual within the Indo-European family in having grown from the language of a defeated and deported subject population to the dominant language among several others within the region to which that population was exiled. How on earth did this happen? Zimansky suggests that the Urartians' policy of importing manpower on a large scale may have served in some ways as a model for subsequent policies of forced relocation within the Near East by contemporary and later imperial powers such as the

Assyrians, the Neo-Babylonians, and the Persians. Are there any other examples of such deported populations becoming dominant in their new homes? For example, might such a model be of service in explaining the location of Tocharian?

* * *

Peter Kuniholm's paper raises some basic environmental and chronological issues that until today, if I am not mistaken, have not figured at all in debates over the nature of Indo-European dispersals but that henceforth can hardly be ignored. The date and nature of a previously unrecognized episode of epic flooding in the Black Sea basin—an event that *must* have had important repercussions on environmental conditions in regions implicated in virtually all reconstructions of Indo-European language dispersal—are clearly of enormous importance. The popularized account of this event published in 1998 by William Ryan and Walter Pitman with the catchy title of *Noah's Flood* dates the inundation in the second quarter of the sixth millennium B.C., reconstructs it as a relatively sudden phenomenon that was completed within a matter of just two years or so, and posits a rise in the water level within the basin of well over one hundred meters, which entails a virtual doubling of the surface area of the body of water that now had metamorphosed into a brackish sea from an earlier state as a huge freshwater lake. Kuniholm's dendrochronological dating of waterlogged timbers recovered from the underwater site of Kiten indicates that a quite different reconstruction may be in order: a good part (but in fact how much?) of at least the uppermost ten meters or so of water evidently did not reach its present level until the early twenty-eighth century B.C. Moreover, the evidence from Kiten—and perhaps this can be supplemented in the near future from other Early Bronze Age sites relatively nearby such as Sozopol and Ropotamo—suggests that the process of flooding, at least by this level, had slowed down enormously, enough so that the inhabitants at this seaside locale struggled over at least two generations, by cutting fresh pilings and thereby raising the absolute elevation of their architecture, against the mounting pressure to abandon their site. How these two substantially different pictures of the flooding can be harmonized, or even if they should be, is at present unclear. But regardless of exactly when the bulk of the flooding took place between eight and five thousand years ago, it would have had a dramatic, indeed probably drastic, impact on environments of singular interest to us at this conference. Relatively flat landscapes around the western and northern shores of the Black Sea would have been radically altered, and the climate, and hence possibly also the vegetation, in regions all around the Black Sea could also have been substantially changed. How significant the effects of these changes may have been on the human populations in this region clearly depends on the chronological duration of the flooding in question, as well as on the absolute rise in water level and the annual rate of this rise. I assume that this rise, no matter how sudden or high, would have had no significant impact on water levels in the Sea of

Marmara or the Aegean, but it might well have had some impact on currents in those adjacent bodies of water and hence potentially on sedimentation processes, marine life, and sea travel by human groups. Think for a moment of all the archaeological literature generated by the research of vulcanologists and other earth scientists on the Bronze Age explosion of the Santorini volcano; imagine how much scholarship may result from the environmental changes inherent in an even greater "natural disaster" such as the flooding of the Black Sea, even if it occurred at a substantially earlier time and possibly affected smaller numbers of people. In other words, brace yourselves for a flood of an altogether different kind!

As for the series of new dates for various Middle and Late Bronze Age pre-Hittite and Hittite buildings at the sites of Kültepe, Acemhüyük, and Kuşaklı that Kuniholm has reported, they are welcome reminders of the hoary antiquity of the written Anatolian languages, in comparison to all other written languages universally recognized as Indo-European other than Mycenaean Greek. Of course, given the pace at which new discoveries are being made these days, even this relatively bland statement needs immediate qualification. First, there is a growing consensus among Mycenologists that the Linear B texts now known to us do, in fact, span a fairly lengthy period of time, from the earliest texts of LM II-IIIA1 or the late fifteenth century at Knossos down to late thirteenth century B.C. texts from such mainland palatial sites as Mycenae, Tiryns, and Thebes.[7] This two-century-long span is roughly equivalent to that which separates so-called Middle and Late Hittite. Moreover, if we accept Margalit Finkelberg's argument for an Indo-European identification of the language written by the people we call Minoans in the script we call Linear A, yet another early Indo-European language with its own time depth of up to four centuries could eventually join Greek and the three Bronze Age Anatolian languages of Hittite, Luwian, and Palaic in a dramatically expanded resource base for the investigation of the earliest documented stages of Indo-European linguistic differentiation.[8]

* * *

Colin Renfrew last night and today has built upon the temporal and spatial framework for Proto-Indo-European (PIE) that he has proposed in a recently published article in the *Journal of Indo-European Studies.*[9] The sequence of three broad temporal stages that he terms PIE phases 1, 2, and 3 (or Archaic, Balkan, and Late) represent his attempt to respond to the reasonable objections made by the Sherratts and the Zvelebils to the Cavalli-Sforza "wave of advance" model for the spread of Indo-European languages that he had advocated in his well-known 1987 book, *Archaeology and Language: The Puzzle of Indo-European Origins.* In fact, his three stages are adopted almost without change from the Zvelebils' own triad of stages proposed in 1990, which they prefaced with the following three explicitly stated assumptions:[10]

1. "that . . . Indo-European language dispersals were not a uniform and continuous process, but a punctuated, multi-stage and repetitive one";
2. "that no widespread population displacements took place in most of Europe during the agricultural transition" that Renfrew and others have connected with the spread of Indo-European languages;
3. "that . . . the Indo-European languages spread at different times and due to different causes, resulting in the distributional palimpsest that we are seeing today."

Renfrew has also argued recently in another article published in the *Cambridge Archaeological Journal*[11] that one of the more distinctive features of Greek, a substantial vocabulary of loanwords characterized by distinctive *-nth-* and *-ss-* suffixes that have traditionally been identified as evidence for a pre-Greek substrate language incorporated into Greek sometime after Greek-speakers made the southern Balkan mainland their home, should rather be reidentified as words adopted from a contemporary adstrate language, namely the language of the Minoan inhabitants of Crete. Of the dozens of questions that I'm sure this audience would like to ask him, let me lead off with three.

First, what should we make of the extensive evidence for western Anatolian (and specifically late Troy II) cultural influence on the Greek mainland beginning ca. 2500 B.C., the so-called "Lefkandi I" culture in the Aegean islands and in east-central mainland Greece, which includes changes in ceramics, metallurgy, architecture, and now also, thanks to Prof. Barber, weaving implements?[12] This episode of archaeological "influence" postdates the end of Renfrew's PIE phase 2; that is, it occurred at a stage when, in his view, if I have understood him correctly, Greek was already well on its way to being differentiated from other daughter languages of his "Balkan PIE." This same wave of western Anatolian archaeological "influence" is detectable at essentially the same time in Cilicia far to the southeast and, presumably radiating from there, as far north as Kültepe and as far east as the margins of northern Syria, where depas-derived ceramic types have been found in some quantity.[13] The late Early Bronze site of Kanlıgeçit to the north of the Sea of Marmara excavated within the past decade by Mehmet Özdoğan bears an uncannily close resemblance to the citadel of Troy II in its overall design and suggests a significant expansion of Troy II material culture in this direction as well.[14] How is this purely archaeological "wave"—surely the one that lies behind the massive discontinuity in material culture that separates Early Helladic II and Early Helladic III on the Greek mainland—to be interpreted if, following Renfrew, one considers Greek speakers to have already been present in the southern Balkans for millennia and to have been linguistically differentiated from their northern neighbors for several centuries? Might not this wave, rather than Minoan contacts from the south of a quite different material kind, have been responsible for the *-nth-* and *-ss-* suffixes in Greek?

Second, even if we agree with Renfrew that the horse and wheeled vehicles as well have played too much of a role in the PIE homeland question and are clearly altogether incompatible with a seventh millennium, south-central Anatolian source, how can we ignore the evidence of wool's history as a word and as an extensively used item of material culture? If wool was not developed and widely used until the earlier fourth millennium B.C., then why do the Anatolian languages, if they split off from the rest of Indo-European more than twenty centuries before, have a cognate word for *wool*?[15]

Finally, how in our different approaches to the question can we all, archaeologists and linguists alike, do a better job of heeding the Zvelebils' insistence on the highly variable, punctuated rather than continuous process of Indo-European language dispersal? Renfrew presents us with three major stages of Indo-European differentiation over a period of six millennia in the enormous portions of Eurasia within which Indo-European languages eventually flourished. But let us, just for a moment, restrict our attention to Anatolia—Renfrew's Indo-European "Urheimat"—during the first millennium B.C. and to Indo-European language movements only. We have:

1. the intrusion of Phrygians from southeastern Europe;
2. the probable deportation, following Zimansky, of Armenians to eastern Anatolia by an elite group of non-Indo-European neighbors;
3. the invasion of the Persians from the east and the incorporation of all of Anatolia into their empire;
4. the invasion of the Macedonians from the west and incorporation of all of Anatolia into a series of Hellenistic empires and kingdoms in which the dominant official language was Greek, in spite of the actual presence of only a small number of Greeks in elite positions;
5. the invasion of Celtic Galatians from the west and their settling into west-central Anatolia in a fashion loosely comparable to the Phrygians of seven centuries or more before.

I have no doubt omitted other movements and peoples deserving mention. What I find striking about this recitation of Indo-European groups criss-crossing Anatolia in both directions is how many different branches of the Indo-European family they represent within a single millennium. I wonder if the situation some two millennia earlier might not have been just as complicated? Against a backdrop of such frequent and well-documented comings and goings across the Anatolian mainland as long as three millennia ago, it only seems reasonable to wonder if we can ever realistically hope to distill historical facts about a process which, if we subscribe to Renfrew's model of the spread of Indo-European languages, began as much as six millennia earlier. Yet, at the other extreme, let us consider the possibility that Crete may have been initially colonized by Indo-European speakers as early as the beginning of the seventh millennium B.C.[16] Thereafter, that island may have remained relatively undisturbed by speakers of other languages for as long as five millennia until its putative invasion by Greek-speaking Mycenaeans in the fifteenth century B.C. If the Indo-European character of the language(s) spoken on Crete in the earlier second millennium B.C. can eventually be demonstrated to the satisfaction of a scholarly majority, we may look forward to learning as much from Crete about early Indo-European as we can ever hope to from the much more heavily traveled, and hence far more linguistically complex, land masses to the north.[17]

Notes

1. E. J. W. Barber, *Prehistoric Textiles: The Development of Cloth in the Neolithic and Bronze Ages* (Princeton, 1991); *Women's Work: The First 20,000 Years. Women, Cloth, and Society in Early Times* (New York/London, 1994).

2. Barber, *Women's Work* (supra n.1), 214–17.

3. Barber, *Prehistoric Textiles* (supra n. 1) 99–101, 299–310, 387–93.

4. J. L. Caskey, "The Early Helladic Period in the Argolid," *Hesperia* 29 (1960): 285–303; C. Renfrew, *The Emergence of Civilisation: The Cyclades and the Aegean in the Third Millennium B.C.* (London, 1972), esp. 103–5, 116; J. B. Rutter, *Ceramic Change in the Aegean Early Bronze Age* (Los Angeles, 1979); J. Forsén, *The Twilight of the Early Helladics: A Study of the Disturbances in East-Central and Southern Greece toward the End of the Early Bronze Age* (Jonsered, 1992); J. B. Rutter, "Review of Aegean Prehistory II: The Prepalatial Bronze Age of the Southern and Central Greek Mainland," *AJA* 97 (1993): 745–97, esp. 763–66; S. W. Manning, *The Absolute Chronology of the Aegean Early Bronze Age: Archaeology, Radiocarbon and History* (Sheffield, 1995), 51–73, 81–86; J. Maran, *Kulturwandel auf dem griechischen Festland und den Kykladen im späten 3.Jahrtausend v. Chr.* (Bonn, 1998), esp. 161–305.

5. Barber, *Prehistoric Textiles* (supra n. 1), 93–94, 101–102, figs. 3.14 (Troy IIg; see also C. W. Blegen et al., *Troy I: The First and Second Settlements* [Princeton, 1950], 350–53, figs. 369, 461) and 3.15 (Aphrodisias, Acropolis Trench 4, Complexes III-II; see also M. S. Joukowsky, *Prehistoric Aphrodisias: An Account of the Excavations and Artifact Studies* [Providence/Louvain-la-Neuve, 1986], 102–5 [figs. 88–95], 605–11 [figs. 437–40, esp. fig. 440]); E. Christmann, *Die deutschen Ausgrabungen auf der Pevkakia-Magula in Thessalien II: Die Frühe Bronzezeit* (Bonn, 1996), 13 (pls. 6, 10), 146–48 (plan 9 [Pefkakia EBA Phase 6, House 324]). Next to the hypothetical loom in the apsidal back room of House 324 at Pefkakia was also found a small pot containing a single terracotta spindle whorl, and a further sixteen whorls of various shapes were found scattered throughout the fill of this room (Christmann, 13 [pls. 44:10, 144:22–38]).

6. I. Tzachili, "All-Important Yet Illusive: Looking for Evidence of Cloth-making at Akrotiri," in *Thera and the Aegean World,* 3 vols., ed. D. A. Hardy, C. Doumas, J. A. Sakellarakis, and P. M. Warren (London 1990), 1:380–89; I. Tzachili, *Yphantike kai yphantres sto Proïstoriko Aigaio 2000–1000 p. Ch.* (Heraklion, 1997).

7. For the lengthy history of Linear B writing at Knossos itself, see J. Driessen, "Le palais

de Cnossos au MR II–III: combien de destructions?" in *La Crète mycénienne*, ed. J. Driessen and A. Farnoux (*BCH Supplément* 30) (Paris, 1997), 113–34.

8. See now also G. Owens, "The Structure of the Minoan Language," *JIES* 27 (1999): 15–55 for an even more enthusiastic endorsement of the Indo-European nature of the language written as Linear A.

9. C. Renfrew, "Time Depth, Convergence Theory, and Innovation in Proto-Indo-European: 'Old Europe' as a PIE Linguistic Area," *JIES* 27 (1999): 257–93.

10. M. and K. V. Zvelebil, "Agricultural Transition, 'Indo-European Origins,' and the Spread of Farming," in *When Worlds Collide: The Bellagio Papers*, ed. T. L. Markey and J. A. C. Greppi (Ann Arbor, 1990), 237–66; A. and E. S. Sherratt, "The Archaeology of Indo-European: An Alternative View," *Antiquity* 62 (1988): 584–95.

11. C. Renfrew, "Word of Minos: The Minoan Contribution to Mycenaean Greek and the Linguistic Geography of the Bronze Age Aegean," *CAJ* 8 (1998): 239–64. For a less controversial overview of the current state of research on the languages spoken on prehistoric (or, because literate, better "proto-historic") Crete published at virtually the same time, see Y. Duhoux, "Pre-Hellenic Language(s) of Crete," *JIES* 26 (1998): 1–38.

12. For recent archaeological assessments of the "Lefkandi I" culture, see the titles postdating 1990 cited above in note 4.

13. P. Z. Spanos, *Untersuchung über den bei Homer 'depas amphikypellon' genannten Gefässtypus* (Tübingen, 1972); C. Podzuweit, *Trojanische Gefässformen der Frühbronzezeit in Anatolien, der Ägäis und angrenzenden Gebieten: ein Beitrag zur vergleichenden Stratigraphie* (Mainz-am-Rhein, 1979); M. Mellink, "The Early Bronze Age in West Anatolia: Aegean and Asiatic Correlations," in *The End of the Early Bronze Age in the Aegean*, ed. G. Cadogan (Leiden, 1986), 139–52. All of these studies are usefully summarized and assessed by Manning [supra n. 4], 81–86.

14. M. Özdoğan, H. Parzinger, and N. Karul, "Kirklarelı Kazilari (Aşağı Pınar ve Kanlıgeçit Höyükleri)," *Arkeoloji ve Sanat* 77 (1997): 2–11.

15. In the revised version of his Richmond paper that is published in this volume, Renfrew has suggested a plausible, if not compelling, way out of the problem that the lexical history of the word for *wool* poses for his identification of south-central Anatolia ca. 7000 B.C. as the place and time from which Proto-Indo-European began to be dispersed. As for the horse, note the recently published evidence derived from DNA analysis for a far more complex history of this animal's domestication than heretofore suspected: C. Vilà, J. A. Leonard, A. Götherström, S. Marklund, K. Sandberg, K. Lidén, R. K. Wayne, and H. Ellegren, "Widespread Origins of Domestic Horse Lineages," *Science* 291, no. 5503 (January 2001): 474–77.

16. As maintained by Owens [supra n. 8].

17. Renfrew's insistence (this volume) on our future need to be considerably "more careful in specifying *which* Proto-Indo-European society or dialect we are speaking of" is salutary, whether or not one follows him in deriving all Indo-European languages from a core area in south–central Anatolia.

DISCUSSION SESSION

SATURDAY AFTERNOON

DREWS Thank you very much, Jerry, for those comments, observations, criticism, expansion. We now have approximately thirty minutes for extended discussion and debate, so I ask for your questions and comments.

GARRETT OLMSTED: I would like to make a comment on a major point that Professor Renfrew made, when he dealt with the mother goddess problem. I think we need to realize that this *Mutterrecht* versus *Vaterrecht* was an idea of Lewis Morgan. Professor Barber's idea of "proto-Us" is something that is useful to look at here because to many of these earlier scholars of the nineteenth century, this Proto-Indo-European culture was a "proto-Us." They wanted to make sure that their ancestors were *Vaterrecht* people. And so they developed theories of what Indo-Europeans were like that would fit into that preconceived concept. But actually—which would fit into what Renfrew is saying—every single Indo-European language group had mother goddesses in its mythology. For example, if we go to Ireland to look at Boand and her sisters, or if we go to Sarasvahti and her sisters in India, they play an extremely important role. I think that if these earlier cultures don't seem to fit in with what Marija Gimbutas was saying, we have to realize that Morgan was looking over Gimbutas's shoulder when she made these comments, and she wanted very deeply to have her ancestors be *Vaterrecht* people.

DARDEN: I've actually been thinking for the last day trying to figure out exactly what it is that archeologists do, at least what some archeologists do when they are trying to relate archeology to language. To tell the truth it's a mystery. Let me first of all say what linguists want. There is a large body of literature and research on the reconstruction of Indo-European, and there are two kinds of reconstruction. There is comparative reconstruction, which basically compares daughter languages and produces a description of the language as it existed at the point where the first language first differentiated, and no further back. And at that point you get all the vocabulary you are ever going to get. You get all the systematic grammar you are ever going to get.

But there is another way of looking at things, which is the internal reconstruction. Once you think you know what Indo-European looked like, then you can look at the grammar as it exists, and to some extent at the morphological derivational patterns, and try to go deeper. And at that point you don't know where you are in terms of what things co-occur with what other things. You are no longer reconstructing a coherent language; you're just hypothesizing about how parts of that language came into existence.

Now the first level of reconstruction includes Hittite or Anatolian, and at that level you already have thematic verbs and thematic nouns. So the allusion to an earlier version of Proto-Indo-European, a version which has only athematic inflection, belongs to this amorphous reconstruction of what things may have come from, and is not related at all to the separation of Anatolian from the rest of Indo-European because Anatolian is included in that part of Indo-European which has thematic inflection.

Since you do have a reconstructed vocabulary, the only thing you can do to relate archeology to the linguistic evidence is to try to figure out what kind of cultures would be reflected by the vocabularies which you reconstruct and look for cultures that reflect that. And that means you can't go any further back than the first stage, which is the stage of the first division. And if you go any further back, you are not relating it to language any more. There has been a long history of argument over whether you can relate culture to language in prehistory, and everybody knows it's dangerous to do that because we know that in modern times cultures and language don't always coincide. But the same sort of principles which delineate boundaries between cultures delineate boundaries between dialects and languages: major rivers, mountain ranges, things like that. So that you have a chance of relating culture to language with only being wrong 20 percent of the time, let's say, or something like that.

But as far as I can see, this "pre-pre-Indo-European" is trying to relate not culture but geography to language. Not culture and not data, but just an assumption that since at some point there are Indo-European languages attested in Anatolia, then they have been there forever. You can't go farther back than the vocabulary that you have, and it's extremely dangerous to make the assumption that because you have language attested at one spot you can go indefinitely back at that spot and assume that those same people were there, or that the same languages were being spoken there.

MELCHERT: Because I am going to have thirty official minutes of response tomorrow, I will postpone a number of linguistic issues until that discussion. But here I do want to respond to just two points in Professor Renfrew's presentation. One is that it is certainly true that the Indo-Hittite hypothesis is quite compatible with an Anatolian homeland. What I must stress, however, is that it is equally compatible with the Gimbutas homeland or, in fact, strictly speaking, with any one of the homelands that were on that map. All that the linguistic argument says is that, to express it in family-tree terms, the Anatolians hived off or separated off first. Unfortunately, that particular kind of linguistic argument cannot tell us who moved away from whom.

The only other thing that I want to mention here is that I must protest against relying too heavily on Professor Carruba's claims about the significance of no substrate beyond Luwian in western Anatolia. In the first place I refer you to a recent work by Frank Starke in *Studia Troica* VII where I think he makes a very interesting argument that the wide extent of Luwian in the narrow sense across all of western and southern Anatolia is due to a very recent (and he means only a few centuries ahead of our texts), a very recent expansion, which very likely overran other divergent Anatolian, Indo-European dialects. Under those circumstances it would not be shocking if there isn't much left of any putative language that was there before that.

My other point is—I can only do a few of these off the top of my head—it is hard for me to imagine even what Professor Carruba's Indo-European and Luwian etymologies are for Lycian place names such as *Awarna* (Xanthos), *Talawa* (Tlos), and *Pinara*. With an excess of imagination one can make anything into Indo-European, but I would protest that there are a substantial number of western Anatolian place names that are not in any particular or striking way Indo-European. And I don't think there is any reason to think that they aren't true non-Indo-European substrate. This again doesn't mean that what you are claiming is wrong; it's just that I do not think that one can base much on this particular argument.

ELIZABETH FISHER: I'm Elizabeth Fisher, from Randolph-Macon College, and I have a question for anyone who knows the answer—and it may be a rather dumb question, I don't know—but in early Egyptian and early Mesopotamian languages, if Indo-European languages have already diverged and gone their various ways, are there imported words that come from the various Indo-European families that can be identified in these other languages?

MELCHERT: The question is not at all stupid. If we had enough of these imported words it would be extremely useful.

FISHER: If we had maybe words from some sort of Greekish language, some sort of Indo-European?

MELCHERT: It has been tried.

FISHER: We can't do it, yet?

MELCHERT: Well, some of us are not satisfied that it can be done usefully. But opinions will diverge, quite radically.

RICH ALDERSON: I would like to address one element of Professor Renfrew's talks last night as well as today, and his apparent reliance on the Taylor-Ringe-Warnow cladistic tree. In the last months there has been a great deal of discussion of this tree on the Indo-European mailing list, on the Internet, of which I am the moderator. The appearance of the tree is that it's been done the way biological cladistic trees are done in the biological sciences. However, it appears that the data that has actually been used assumes some of the conclusions that are being made. For example, it has built into it the "Anatolian as first split."

Also, cladistic trees are unrooted. You can pick your root at any point. So if, for example, we start at the middle of the tree, as typically drawn, as the split point, then Anatolia went off way off over here and at the bottom, not up at the top. Those are just some of the things that need to be taken into account when using that tree.

DREWS: I have a question for Paul Zimansky. One of the issues, I think, in your paper is the writing of Urartian, the use of Urartian (or what we call Urartian) for inscriptions from the ninth century through the seventh. Usually the claim is made, relative to Armenian, that Armenian is obviously a latecomer to that part of the world because Armenian was not the language written by these kings. Armenian was never written until the fifth century A.D. That argument has always seemed to me very fragile, and what you are suggesting, I think, is that the whole concept of writing was new in eastern Anatolia. They hadn't done it before. An inscription that was published two years ago indicates that even the name of Sarduri was spelled in two different ways in the first generation, evidently indicating a certain freshness and naiveté in the written expression of that language. What I am wondering is, do you think that the Urartian language itself, related to Hurrian, could have been imported by this dynasty as part of the cultural assemblage that came in with everything else, including the worship of Haldi, the main divinity? In other words, is there any reason to assume that Urartian was a native language of that area?

ZIMANSKY: No there isn't any reason to assume that it's native to the Van area. No evidence one way or the other. The introduction of writing follows a very standard pattern in Urartian. The same sort of thing we see in Hittite. They start off using writing when they create their kingship. They also borrow wholesale from Assyria. The earliest Urartian inscriptions are completely cribbed from neo-Assyrian royal inscriptions: just the names of the kings and a few of the place names change, but otherwise verbatim. The only thing that strikes us as a little odd is that one of the earliest inscriptions of Sarduri, on a great big stone block repeated six times, begins with the sign for *tablet*: "this tablet was written by so-and-so." The scribe didn't have it quite right. And the Urartians *do* do specific things: They use the word for *palace* a little differently.

But the problem with trying to figure out anything about the Urartian language is that these texts are so formulaic. Everything is repeated over and over again, and they don't say very many different things initially at all. Particularly the late ninth- and eighth-century inscriptions are virtually identical. You don't see any linguistic development.

Now where you do see a real difference is in the handful of tablets that have been excavated. We have no clay tablet from before the reign of Rusa II. Whether they weren't writing then, we don't know. But archeologically the only ones we can date belong to the reign of Rusa II. And those are almost in a different language. They also use different sign forms. For instance in basic lapidary Urartian the word for *king* is written with two winkelhakens, like the number twenty. On tablets they use the much more complicated LUGAL sign that is customary in Mesopotamia. It's as if the scribal tradition for the Urartian tablets is different, and I think there is no reason why we shouldn't drop down into a room somewhere with 10,000 Urartian tablets in it. They seem to be doing this in the time of Rusa II. And if we make such a find, we'll know a lot more about Urartian linguistically. But at the moment we just have these very formulaic royal inscriptions that aren't telling us very much about language.

DREWS: As organizer I get to ask two questions, so now I'd like to ask Professor Renfrew to clarify something. I think I understand your position on this, but what you are really saying is that in the Aegean we have an evolution that moves on two tracks, both coming from an importation in the seventh or possibly eighth millennium B.C. One track leads in the Greek mainland to the evolution of Greek, by the time of the Linear B tablets. The other evolution, just down the way in Crete, leads to this very different language that Professor Margalit Finkelberg will be talking about soon. But is that in fact your position, that we have two very divergent evolutions proceeding over the course of about five thousand years and ending up in such divergent positions?

RENFREW: Well yes, more or less. I am assuming, of course, as it has been suggested very reasonably, that there may be more than one language in Minoan Crete; that's a point that's been made, and I think it's very difficult to contradict the possibility. Yes, I do assume that the earliest Proto-Minoan language probably came to Crete with the first farmers around that time, and I do assume that the language that came to mainland Greece with the first farmers at the same time was antecedent to Greek, although Greek will have participated in the Balkan *Sprachbund.* But there is more to it than that, because if you are looking at the Aegean you clearly have the languages of the west Anatolian coast, which are no doubt Anatolian languages, but the point has just been made and very validly—made in two different ways—that Luwian may be the end of the story, as it were, on the west Anatolian coast, until Ionian Greeks arrived, which I don't doubt, late in the Mycenean period. So there is the history of what was going on in west Anatolia, which of course led, by the first millennium B.C., to a series of languages in west Anatolia. So that is quite complex.

And then, equally, we mustn't forget the north Aegean. If we've got Thracian and Dacian there, at the time of our earliest evidence, we may have complexities there. Then, I think Jeremy Rutter's point is a very valid one, that we no doubt have other early groups. In particular, at the end of the Early Bronze Age, at Lefkandi and at Kastri in Syros, there are well-recognized archaeological complexes, which may come from the north Aegean. Would we not expect there to be some linguistic correlation there? We do not want to rely too heavily on the view that culture equals language, as Professor Darden cautioned us, but nevertheless, when you have cultural change in a pronounced way, we may expect some language change. So I think Jeremy Rutter's point is a very valid one. So the dichotomy, as I first emphasized it, is no doubt oversimplified. But it still may be the basic division, so long as one doesn't forget the Anatolian coast, which is a very important part of the area. And we shouldn't forget Leonard Palmer's attempts to suggest that Luwian or early Luwian accounted for a great deal of what was going on in the Aegean in the place names, and so on. So there is a third component to the two you mention, which is whatever has happened in west Anatolia over that time period.

IVANOV: I would like to draw attention to the linguistic part of the question about the age of the coming of Armenian. Long ago Kapantsan and some other Armenian scholars published important studies on Hittite borrowings in Armenian, but, unfortunately, they wrote in Armenian, only partly in Russian. Thus these studies with many data about possible Hittite influence on Armenian have not been paid much attention to. Some examples seem quite clear, like, for instance, the words which are still used in modern Armenian. If you visit Lake Sevan, you may taste the wonderful fish there, which is called the king [fish], that is, *ishkhan*. This is clearly Hittite *eshkha-* or *ishkha-* (master) And many words like these are definitely loan words. For instance, this *ishkhan* has a laryngeal *kh*, which cannot appear in Armenian in the middle. So linguistically these are borrowings. And if there was a contact of Hittite and Armenian, definitely Armenian should have been spoken somewhere in Anatolia. Otherwise it would be simply fantastic.

Then there is the problem of the name. Modern Armenian is called *Hayastan*. The *-stan*, of course, is a late insertion. But *Haya* is a very old term, and Gelb published a cuneiform tablet that points to the existence of some ethnonym, *Haya*, as some not quite identifiable place near Anatolia on the border of the third and second millennium. And there are Hieroglyphic Luwian inscriptions on metal, and on these Kululu lead strips there is also this word *Haya* in the enumeration of different names of peoples and tribes. So I think that *Haya* definitely existed very early, in the period that might be interesting for us here. There are also different and other reasons to suppose that Armenian really was one of the Indo-European languages spoken near Anatolia or in eastern Anatolia around the second millennium B.C.

LEHRMAN: With regard to the name of Armenians, I would just like to add a note of caution. The transfer of names is a very well known phenomenon. For example, if you were to equate the language of the Franks and the contemporary name of the country France, we would get into big trouble. The same thing goes for the ethnonym of *Russians,* which belonged originally to a southern Swedish Viking tribe, and again the language is far from identical. So in this time depth we cannot at all be sure that *hayasha* mentioned in the text Professor Ivanov just referred to are the same *Haya* who called their language *leza hayotz* and who did happen to speak Armenian.

IVANOV: I am referring to *Haya.* The term *Hayasa* is another thing. *Hayasa* is probably a different name. But the name I referred to in Gelb's tablet and in these Luwian texts is just *Haya or Ha-*.

LEHRMAN: It's still the same in principle.

JOHN GREPPIN: The word *ishkhan* doesn't mean *king;* it means *prince.* Also it's more than likely a Persian loan word, *khshathra* being the ultimate origin. As for *Hayasa,* I think the etymology is terribly, terribly weak because intervocalic *y* couldn't have maintained itself in Armenian. Frankly, phonologically, it could be related to *hatti,* where intervocalic *t* produces a *y* very nicely.

IVANOV: I am speaking about *Haya-* and there is no *-s-* in this form. There is no phonetic reason to explain why this *Hatti* should become *Haya.* There is large literature on the topic.

DREWS: Well, our Armenian specialists can argue this out over the coffee break. We will reconvene in about fifteen minutes.

[break]

THE LANGUAGE OF LINEAR A

GREEK, SEMITIC, OR ANATOLIAN?

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It is generally recognized today that the overwhelming majority of the signs used in the Linear A script have graphic equivalents in the Linear B syllabary. The situation was different in 1945, when E. Pugliese Carratelli introduced the first classification of Linear A signs and their suggested Linear B parallels. In 1961 W. C. Brice proposed a modified version of the Carratelli system, based on a much wider range of Linear A sources; however, no Linear B equivalents to the Linear A signs were suggested in Brice's edition. It was only in 1985, in the concluding volume of their new and thorough edition of Linear A texts (*Recueil des inscriptions en linéaire A*, henceforth *GORILA*) that Louis Godart and Jean-Pierre Olivier introduced a joint numeration of the Linear A and Linear B signs based on E. L. Bennett's standard numeration of the signs of Linear B, thus having recognized for the first time the essential mutual commensurability of the two scripts (Carratelli 1945: 422–610; Brice 1961: table 1; Godart and Olivier 1985: xxxvi).

But even before the publication of *GORILA*, the scholars who approached the Linear A script with the phonetic values of Linear B succeeded in producing an impressive list of words (presumably, proper names and the names of commodities) identical to words encountered in Linear B texts, especially those of the Knossos tablets. The significance of this achievement is hard to overestimate. Indeed, if the name *ku-ka-da-ro*, which appears in a Knossos Linear B tablet in a context leaving no doubt that this is a proper name, emerges as *ku-ka-da-ra* in a similar Linear A context, and if the same is true of a considerable number of other words (see fig. 1), this can hardly be due to casual repetition of identical clusters of signs.[1] Scribal alternations within the Linear A script itself point in the same direction. Thus, two Hagia Triada tablets, HT 95 and HT 86, contain identical lists of words, the only difference being that the word *qe-rya-u* of HT 95 is represented on HT 86 as *qa-rya-wa* (fig. 2). The identity of the contexts in which the two appear leaves no room for doubt that *qe-rya-u* and *qa-rya-wa* are nothing else but two variant spellings of one and the same word. That is to say, the difference between the two is best accounted for as some kind of phonetic alternation between *qe*/*qa* and *u*/*wa* (cf. Karetsou, Godart, and Olivier 1985: 128 and n. 58). The same explanation holds

Fig. 1. Sample of Linear B–Linear A Parallels

Linear B	Linear A
ku-ka-da-ro	*ku-ku-da-ra*
pa-i-to	*pa-i-to*
ku-mi-na	*ku-mi-na*
di-de-ro	*di-de-ru*
qa-qa-ro	*qa-qa-ru*
a-ra-na-ro	*a-ra-na-re*

Fig. 2. Phonetic Alternations within Linear A Texts

HT 95	HT 86
da-me	*da-me*
mi-nu-te	*mi-nu-te*
sa-ru	*sa-ru*
ku-ni-su	*ku-ni-su*
di-de-ru	*di-de-ru*
qa-rya-wa	*qe-rya-u*

true for the *a/ja* doublet, which is abundantly attested in Minoan. It goes without saying that such alternations could only be possible if the Linear A/Linear B graphic equivalents involved were assigned identical or closely similar phonetic values.

Comparison of the two Aegean scripts on a more general scale leads to the same conclusion. It is important to take into account in this connection that the Minoan script possesses a considerably lesser amount of signs than its Mycenaean counterpart.[2] A simple count of the Linear A signs as placed within the new AB series in the *GORILA* edition shows that they cover only sixty-four of the eighty-nine signs in Bennett's Linear B list. Taking into account the signs that are used in Linear A only (*GORILA*'s A-series) increases the total by no more than a few items, because these are mainly commodity signs that do not form part of Linear A words. In terms of phonetic values, the Linear B signs not represented in Linear A fall into three categories: (a) complex syllables (a_2, *rai*, *ai*, *nwa*, *pte*, ri_2, *two*, ro_2, *dwe*, *dwo*), (b) syllables of the *o*-series (*so*, *do*, *mo*, *qo*, *jo*, *wo*, *no*, *two*, ro_2, *dwo*, and probably also *zo*),[3] and (c) syllables that do not belong in either of the two former categories (*pe* and *we* in *GORILA*). Clearly, only the latter category can be thought of as due to the hazards of representation. As far as the two former are concerned, the absence of the complex syllables probably indicates an earlier stage in the development of the script, whereas the systematic underrepresentation of the *o*-series obviously calls for a phonological explanation. This proves once more the applicability of the Linear B phonetic values to the signs of Linear A.[4]

Still, approaching Linear A with the phonetic values of Linear B has led to hypotheses as mutually exclusive as the Greek hypothesis launched by Vladimir Georgiev, the Semitic hypothesis initiated by Cyrus Gordon, and the Luwian hypothesis first formulated by Leonard Palmer, to mention only the more influential arguments. This difference among opinions seems to arise from the fact that the etymological approach has been the main, if not the only, method used to analyze the language of Linear A. Drawing one's conclusions (or aruguing) from simple outward resemblances with words in other languages can, however, hardly be regarded, in this context, as methodologically satisfactory. This point was made especially clear by Yves Duhoux in the course of the discussion "Linear A as Greek" that took place in AEGEANET in March 1998:

> I would like to remind you of some basic facts related to the Greekness of Linear A's language: (1) The word for "total" is different in Linear A and in Linear B: LB *to - so*(*- de*); LA > B *ku-ro*. (2) The Linear B language is significantly less "prefixing" than Linear A. (3) Votive Linear A texts, where we are pretty sure to have variant forms of the same "word," show morphological (I mean : grammatical) features totally different from Linear B.
>
> The conclusion must be that even if one can find some casual resemblances between words in both languages (remember this MUST statistically happen: e. g. English and Persian use the same word "bad" to express the meaning of BAD, although it is proven that both words have no genetic relation at all), they are probably structurally different.

That is to say, no identification of an isolated word on the basis of its outward resemblance to a word in another language, attractive as it may appear, can serve as proof of the identity of the language in question until its overall phonological and especially morphological system has been reconstituted. This is not, however, what happened to the language of Linear A.

Let us take for example the Semitic hypothesis. In a recently published article Duhoux drew attention to the fact that, notwithstanding that the Linear A *ku-ro* (total) exactly corresponds to the Semitic *kl* (total), "this same Semitic hypothesis proves unable to satisfactorily analyse LA > B *po-to-ku-ro*, meaning 'grand total.' This is unfortunate, and shows how quickly an interpretation may reveal its limits" (Duhoux 1998: 33). It is important to realize in this connection that most of the phonetic values with which Cyrus Gordon worked almost half a century ago would be regarded today as unacceptable, not to mention the fact that Gordon got lost in the archives of Hagia Triada, reading terms for transactions as personal names, and so on. Jan Best's Semitic interpretation of the language of Linear A is along the lines of Gordon's hypothesis and, like Gordon's, Best's method is mainly etymological. In addition, Best's hypothesis is far from flawless from the epigraphical point of view as well. Not only does Best completely ignore the now standard edition of the Linear A texts by Godart and Olivier, but some of his readings of the Linear A signs are sharply at variance with the scholarly consensus as to their identity. Thus, for example, the sequence read by him as *a-ta-nu-ti* and analysed as "the verbal stem *a-ta-nu* followed by the ending of the first singular person in the perfect *-ti*, 'I have given'" would be read as *a-ta-i-301-* by most Linear A scholars.[5] If Best has reasons of his own for proposing this and similar readings, he does not share them with his readers. All this seems to indicate that in order to become a respectable scholarly proposal, the Semitic hypothesis should be reformulated on entirely new grounds. The same would apply to the Hurrian hypothesis, which seems to be also mainly based on the etymological approach.

The Luwian reading proposed by Palmer for a fragmentary inscription on a libation table from Knossos presents a problem of a different kind. Palmer's reading is, to a large degree, based on his identification of the sequence *ija* in this inscription with the dative case of the third-person enclitic pronoun related to Lycian *-ije*.[6] Yet, to interpret Minoan *ija* in such a manner is to ignore the fact that the Luwian form of the third-person enclitic pronoun is *-tu*, not *-ije*. Accordingly, Palmer's hypothesis lacks in methodological coherence, in that it simultaneously uses mutually exclusive morphological data from two related but nevertheless different languages. Palmer, however, has not been the only one to maintain that the language of Linear A should be identified with Luwian or with some other language of the Anatolian group. Hittite was proposed by Simon Davis, Lycian by myself, and Luwian again by Edwin Brown (Davis 1967; Finkelberg 1990/91; Brown 1992/93). Indeed, it cannot be denied that Anatolian languages are rapidly gaining in popularity in the Linear A literature. As far as I can see, this phenomenon is partly due to the increase in our knowledge of these languages and partly to our new estimation of the linguistic situation in the eastern Mediterranean in the second millennium B.C. Since the latter factor is historical in nature, I will discuss it in the concluding part of this paper.

From the purely linguistic point of view, however, it seems clear enough that no a priori identification of the language of Linear A, plausible as it may appear for cultural and historical reasons, can stand the test of methodological coherence. Only when we can show that the phonological and morphological regularities displayed in unknown language X agree with those of known language Y, only then shall we be in the position to formulate a hypothesis as to the identity of the language in question. What follows is an attempt to draw the phonological and morphological profile of the language of Linear A and to compare this profile with those of other languages, including those identified in the past with the language of Linear A. I hope that this approach will allow us to reduce the range of the linguistically available possibilities and also, perhaps, to indicate the direction in which the identification of the language of Linear A should proceed.

The Phonological Profile of Minoan

In his "Mycénien QAQARO / minoen QAQARU," which appeared as early as 1956, Michel Lejeune threw light on two significant features in Minoan word-building: reduplication and the abundance of *u*-stems. Especially has Lejeune's list of Linear A–Linear B doublets, such as *a-ku-tu*/*a-ko-to*, *di-de-ru*/*di-de-ro*, *ka-ru*/*ka-ro*, *ma-ru*/*ma-ro*, *qa-qa-ru*/*qa-qa-ro*, demonstrated beyond doubt that the Linear A *u*-endings are answered by *o*-endings in the parallel Linear B words (Lejeune 1972: 203–9). Obviously, this feature of Minoan phonology points in the same direction as underrepresentation of the *o*-series in the Linear A syllabary, indicating again that the characteristic feature of Minoan vocalism is a system of vowels based on *a*, *u*, *i*, and *e* only.

The characteristic feature of Minoan consonantism is the lack of distinction between voiced and voiceless stops in the velar and labial series. In view of this, it is highly questionable whether the so-called *t*/*d* distinction reflected in the Linear A, Linear B, and Cypriot syllabic scripts is indeed the one between the voiced and the voiceless dental stop. It is much more reasonable to suppose that the distinction in question stood for some kind of opposition within the dental series itself (for example, between the occlusive and the fricative dental consonant, as proposed for Luwian by Morpurgo Davies, or between two different *t*-sounds as proposed for Hattian by Kammenhuber [Morpurgo Davies 1982/83: 252, 256; Kammenhuber 1969b: 449]) and that it was used in Linear B for rendering the distinction between the voiced and voiceless stop inherent in the Greek language. Otherwise, it would be difficult to explain why the *t*/*d* distinction was abandoned in the Cypriot syllabary and why the same Linear A sign is read as *da* in the Linear B and as *ta* in the Cypriot syllabary.[7]

To recapitulate, the characteristic features of the phonology of the language of Linear A—its phonological profile, as it were—seem to have been the system of vowels based on *a*, *u*, *i*, and *e* and the lack of distinction between the voiced and voiceless stops. Interestingly enough, application of these parameters to the extant hypotheses concerning the identity of the language of Linear A shows that most of them would not stand the test.

Let us start with the Greek hypothesis. As already mentioned, almost half a century ago Michel Lejeune showed in a simple and elegant way that while Greek is an *-o* language the Minoan is an *u*-language and is therefore not likely to be Greek. A simple comparison with both the Linear B and the Cypriot syllabary, both of which abound in the vowel *o*, let alone our knowledge of the vocalism of historic Greek, shows, in fact, two mutually incompatible systems of vowels. Comparison of the consonantal systems of both languages leads to a similar conclusion. The opposition between voiced and voiceless stops is inherent in the Greek language. We saw, however, that the language of Linear A does not possess this kind of opposition. Needless to say, the lack of agreement on these two points actually excludes Greek as the language of Linear A.

Let us turn now to the Semitic hypothesis. The conclusion that Minoan vocalism was based on *a*, *u*, *i*, and *e* can hardly apply to the vowel-system of the Semitic languages, which possessed only the vowels *a*, *u*, and *i*.[8] Comparison with Semitic consonantism also yields negative results: although some early Semitic cuneiform sources are inconsistent in rendering the *tenues*/*mediae* opposition, linguists generally agree that this is because the borrowed Sumerian system of writing, unfit as it was for rendering Semitic consonantism, failed to reflect this distinction until some later stage (see Moscati et al. 1980: 23–46, esp. 26, 31, 37; cf. Sturtevant 1951: 3). Since the opposition between voiced and voiceless stops is inherent in Semitic languages, it is hard to believe that a script including no such opposition could be an indigenous Semitic script. Thus, the lack of agreement in both the vocal and the consonantal system practically excludes the possibility that the language of Linear A can be identified as a Semitic language.

Application of the same criteria to the Anatolian languages shows that although the above description of Minoan vocalism fits several Anatolian languages, such as Hittite, Palaic, or Lycian, it does not support the hypothesis that the language of Linear A should be identified as Luwian. Luwian stands apart from the other Anatolian languages in that it does not possess the phoneme *e*, its vocalism being based on the vowels *a*, *u*, *i* only (cf. Melchert 1994: 25 [CLuwian], 38 [HLuwian], 239–45). This phonological peculiarity of Luwian actually excludes the possibility that the language of Linear A can be identified as Luwian. As far as the consonantal system is concerned, the lack of distinction between the voiced and the voiceless stops which, as we saw, is characteristic of Minoan, is in full compatibility with what we find in the Anatolian languages. The cuneiform languages—Hittite, Palaic, and Luwian—are manifestly inconsistent in using this distinction found in the Akkadian cuneiform they borrowed, and Hieroglyphic Luwian, which is after all an indigenous Anatolian script, does not possess it at all. The same would be true of the so-called "Sturtevant law," according to which the cuneiform Anatolian languages distinguished between voiced and voiceless stops by means of single or double writing of the same consonant, a practice borrowed from Hurrian. "The synchronic distinction in the cuneiform languages must have been something *other* than voicing; otherwise there is no way to explain why they did not simply use the contrasting voiceless/voiced signs of the Akkadian syllabary. Rather, they borrowed from Hurrian the device of -CC- vs. -C-. Despite great debate about just what the synchronic phonetic difference is (fortis/lenis, aspirated/unaspirated, long/short), the one thing it surely is not is voicing."[9] Note also that the documents in the Classical Age Anatolian languages, such as Lycian, Lydian, and Carian, are not consistent in rendering the *tenues*/*mediae* distinction imposed on the speakers of those languages by the Greek alphabet they used.[10] All this indicates that the lack of distinction between voiced, voiceless, and aspirated stops is indeed an authentic feature of Anatolian consonantism.

Thus, our drawing of the phonological profile of the language of Linear A, though provisionary, is effective enough to rule out Greek, Semitic, and Luwian from the list of languages potentially relevant to the language of Linear A. Again, the vocalism of Linear A is based on *a, u, i,* and *e* and is therefore incompatible with the vocalism of Greek, Semitic, or Luwian. Likewise, the consonantism of Linear A is characterized by the lack of distinction between the voiced and voiceless stops and is therefore incompatible with the consonantism of either Greek or Semitic (see fig. 3)

Fig. 3. The Phonological Profile of Minoan

	Minoan	Greek	Semitic	Luwian	Hittite Palaic Lycian Hattian Sumerian
vowels	a, u, i, e	a, u, i, e, o	a, u, i	a, u, i	a, u, i, e
ten./med.	absent	present	present	absent	absent

Note that application of the latter criterion would also exclude Hurrian, proposed by some as the language of Linear A, because this language definitely distinguished between the voiceless and voiced stops.[11] It does not however exclude Anatolian languages other than Luwian, nor some non–Indo-European languages, such as Hattic or Sumerian, nor does it exclude Etruscan. Both the vocal and the consonantal systems of all those languages seem indistinguishable from those of Minoan. The situation is therefore similar to that described by Barber:

> Once again we are faced with the problem that we are best at disproving, and only . . . , by the accomplishment of decipherment itself, capable of proving. This fact does not mean, however, that we can learn nothing useful until the actual decipherment. The very fact that certain languages do not seem to be good bets, from a structural point of view, will be of considerable value to the archaeologist. Indeed, if there are too few data in the corpus for any hope of full decipherment, these implications may be the most far-reaching results we can obtain from the whole investigation (Barber 1974: 215).

The question is, of course, whether by pursuing a method that proceeds from drawing a coherent picture of the language of Linear A we can also arrive at positive results. The answer directly depends on whether the material at our disposal is sufficient to make a morphological interpretation possible.

A Tentative Interpretation of Some Minoan Texts

The extant Linear A sources fall into two clearly distinct groups. The larger part of our corpus is formed by the tablets of the Hagia Triada archives. This group of texts is generally seen as belonging to Late Minoan Ib (ca. 1500—1450 B.C.). What remains consists of tablets from other archives of which only the recently discovered Khania tablets form a consistent group, of graffiti and occasional inscriptions on clay vessels and metal objects, and finally, of inscriptions on libation vessels of stone, from different Cretan sites, traced back to the beginning of Middle Minoan IIIb (ca. 1700 B.C.). Although the Hagia Triada tablets provide sufficient material to permit a systematic study of Minoan phonetics, word building, and especially onomastics, the fact that these texts are probably no more than lists of names and commodities participating in various transactions prevents them from being taken as a starting point of morphological analysis. As distinct from this, the inscriptions on libation vessels, few as they are, form the only group of Linear A texts that allows for application of the combinatorial method, in that at least some of them give us the opportunity of analysing complete syntactic units. If, as a result of such analysis, we succeed in isolating the morphological elements that form Minoan sentence structure, this, together with the above conclusions about the characteristic features of Minoan phonology, can give us a reliable basis for identification of the language of Linear A.

The majority of the inscriptions on libation vessels whose beginnings have been preserved open with a group of signs reading *a-ta-i-301-wa-ja*, and several inscriptions open with remarkably similar sequences: [12]

1. *a-ta-i-301-wa-ja* (10 times)
2. *ja-ta-i-301-u-ja* (once)
3. *a-ta-i-301-wa-ẹ* (once)
4.]*a-na-ti-301-waja* (once)
5. *ta-na-i-301-u-ti-nu* (once)[13]
6. *ta-na-i-301-ti* (once).

Considering that both the *a/ja* and the *u/wa* alternations are firmly attested for Minoan, we can safely take such hapaxes as *ja-ta-i-301-u-ja* (AP Za 1) or *a-ta-i-301-wa-ẹ* (PK Za 11) (nos. 2 and 3) as phonetic or graphic variants of the more widespread no. 1 (cf. Karetsou, Godart, Olivier 1985: 128; Metaxa-Muhly 1984: 126). Distinct from this, the sequences found under nos. 4, 5, and 6 obviously represent semantic rather than phonetical variations of the introductory formula.[14] Let us now try to analyse these sequences as a group.

One can see that while both the beginning and end of the introductory formula can vary, its middle, represented by the combination of signs *-i-301-*, is stable. The fact that in the case of]*a-na-ti-* (no. 4) the *i* of the invariable sequence *-i-301* is part of the syllabic sign for *ti* seems to suggest that the sequences *a-ta-i-*, *ta-na-i-*,]*a-na-ti-*

end in the same way, which may be due to the fact that *i* stands for an element in Minoan nominal or verbal flexion. In view of this, we can try to divide the introductory formula in such a way that its first part would end with *-i* (*a-ta-i-*, *ta-na-i-*,]*a-na-ti-*), whereas its second part would begin with A 301 (*-301-wa-ja*, *-301-u-ti-nu*, *-301-ti*).

Note now that the second part of the formula, which invariably begins with sign A 301 (this sign has no equivalent in the Linear B syllabary), does not likely stand for different forms of the same word. The element *u*/*wa*, common to nos, 1, 2, 3, 4, and 5, is absent from no. 6; the element *-ja*, common to nos. 1, 2, 3, and 4, is absent from nos. 5 and 6; the element *ti*, found in nos. 5 and 6, is absent from nos. 1, 2, 3, and 4; whereas the element *-nu* is only characteristic of the sequence represented under no. 5. Obviously, the range of variation involved is too high to be accounted for as due to nominal or verbal flexion. On the other hand, the identical beginning and the recurring elements *u*/*wa* and *ti* make it equally unlikely that the sequences *-301-wa-ja*, *-301-u-ti-nu* and *-301-ti* could stand for three different words entirely unrelated to each other. This can only lead to the conclusion that rather than standing for a single semantic unit, or a "word," the sequences in question present conglomerates of such units, that is, that they are a series of independent semantic elements that can be arranged in more than one way. As far as I can see, the only linguistic phenomenon that can account for such a variably arranged series of independent semantic elements is a combination of particles.

Although combinations of particles are found in many ancient languages, both Indo-European and otherwise, the chain of particles and enclitic pronouns at the beginning of the sentence is the distinctive feature of Anatolian languages of the Indo-European family. To quote Annelies Kammenhuber, "The Hittite-Luwian languages acquire their distinctive physiognomy owing to *the chains of introductory particles, which were being attached to the first word of the sentence as the need arose.*"[15] As both Kammenhuber and Carruba pointed out, this feature of the Anatolian chain of particles opposes Anatolian to all the other languages employing enclitic particles, both Indo-European (such as Greek and Indo-Iranian) and non–Indo-European (Hattic, Hurrian, etc.) (Kammenhuber 1969a: 252–53; Carruba 1969: 107–8).

We saw that the phonological profile of Minoan can fit such a diverse range of languages as Anatolian languages other than Luwian, as well as Hattic and Sumerian, and probably also some others. If the above analysis of the Minoan introductory formula as a chain of particles and enclitic pronouns makes sense, this will allow us to exclude all the languages other than Anatolian from the list of languages that may be relevant to the language of Linear A. This is not yet to say that this fact alone is sufficient for identification of the Minoan sequences of independent elements with the Anatolian chain of particles. To put such identification on solid ground one should be able to demonstrate that the elements A 301, *u*/*wa*, *ti*, *nu*, and *ja* correspond to Anatolian particles in both form and sequence.

The chain of particles and enclitic pronouns opening the Anatolian sentence is nothing less than its morphological and syntactical "code." The particles usually follow the same fixed order: (1) the conjunction *and* or *but*; (2) the particle of quoted speech *-wa-*; (3) the reflexive pronoun *-ti-* (*-za-* in Hittite);[16] (4) personal pronouns; and (5) particles indicating position. Except for the conjunction *and/but*, any one of the elements can be omitted so as to conform to the message of the sentence; in that case, the particles actually present would follow the same order as described above.

If we compare this scheme to the order of the elements in the Minoan sentence, we shall see (1) that the Minoan *u/wa* is identical to the particle of quoted speech *-wa-*, attested for all Anatolian languages (although it apparently functions here as an introductory particle, a practice that is paralleled in Hieroglyphic Luwian; see Meriggi 1962 and Kammenhuber 1969a: 172); (2) that the Minoan *ti* is identical to the reflexive pronoun *-ti-*, attested for Palaic, Luwian, and Lycian; and (3) that their respective positions in Minoan agree with the position of these elements in the Anatolian chain of particles (but cf. above, n. 16 on the position of Hittite). This being the case, we seem to be justified in identifying the elements *-nu* and *-ja*, both of which occupy the place at which in an Anatolian language one would expect to find a personal pronoun, with the accusative case of the third person enclitic pronoun, whose distinctive feature in the Anatolian languages is the sonant *n*, and with the dative (or locative) case of the same pronoun as attested in Lycian *(i)je*.[17] Finally, A 301, which opens the Minoan sequence, functions in exactly the same way as the conjunctions *and* and *but*, invariably placed at the beginning of the Anatolian chain of particles.[18] The correspondences in question can be found in figure 4. Note especially the close similarity of Minoan *-u-ti-nu* with Luwian *-wa-ti-(a)n*.

Fig. 4. Minoan and Anatolian Chains of Particles

Minoan	Hittite	Luwian
-301-*u-ti-nu*	and/but-*wa-za-an*	and/but-*wa-ti-(a)n-*
-301-*wa-ja*	and/but-*wa-ssi*	and/but-*wa-tu/du*
-301-*ti*	and/but-*za*	and/but-*ti*

To sum up, the second part of the introductory formula lends itself to analysis in terms of a combination of particles displaying a number of significant correspondences with the Anatolian chain of particles, namely, attachment to the first word in the sentence, several phonetically identical or similar elements, and the order of their arrangement. Note, however, that in most Anatolian languages the introductory particles are regularly placed at the beginning of the second sentence, whereas the regular Minoan usage consists in placing the introductory particles after the first semantic unit of the sentence that opens the discourse.[19] This practice is widely paralleled in Lycian, as for example in the formula *ebêñnê kupâ me ne* (or *mê ti*) *prñnawatê*, etc., transposed as *this tomb* (acc.), *but-it* (acc.) (or *but-for himself*) *has built*, etc., usually found at the beginning of Lycian inscriptions. It is also worth mentioning in this connection that the quotation in the language of Kftjw (presumably Crete) adduced in an Egyptian papyrus of the eighteenth dynasty (ca. 1580–1350 B.C.) begins with the sequence *santi kapupi waja* (Friedrich's transcription),[20] which is highly similar in structure to the Minoan introductory formula in that it contains an opening unit ending in *-i* and, especially, the element *waja* placed, as in Minoan, after the first semantic unit of the sentence that opens the discourse.

It is important to realize that the ostensibly unconspicuous character of the particles does not render the evidence they supply insignificant. On the contrary, since the way in which a given language makes use of particles is peculiar to that language alone, in everything concerning identification of the language in question the evidence provided by the particles is of first-rate importance. The uniqueness of their location, of their material form, and of the way in which they combine with one another turn the particles into actual "fingerprints" of the language or the family of languages to which they belong.[21] Consequently, the conclusion that the second part of the Minoan introductory formula can be analysed as a chain of particles of the Anatolian type gives us a good reason for further comparison of the language of Linear A with Anatolian languages. With this in view, let us turn again to the first part of the introductory formula.

We saw that the first part of the introductory formula comprises the groups of signs read as *a-ta-i-*, *ta-na-i-*, *]a-na-ti-*. It was suggested above that the ending *i*, common to all the variants, may stand for an element in Minoan nominal or verbal flexion. The only Anatolian ending that would fit these forms seems to be the dative/locative case ending *-i*, common to most languages of the Anatolian group. In view of this, the introductory formula as a whole can be tentatively rendered as follows:

> *a-ta-i-301-wa-ja* (To/in *ata-*, and/but-thus-to/in it [also: to him, here] . . .)
> *]a-na-ti-301-wa-ja* (To/in [. . .] *anat-*, and/but-thus-to/in it [also: to him, here] . . .)
> *ta-na-i-301-u-ti-nu* (To/in *tana-*, and/but-thus-for himself-it/him . . .)
> *ta-na-i-301-ti* (To/in *tana-*, and/but-for himself . . .)[22]

If *a-ta-i-*, *ta-na-i-*, *]a-na-ti-* is the dative, these words would designate either the libation vessels on which the inscriptions were made or the deities to whom they were dedicated; if it is the locative, these words would rather designate the places where the libation vessels were consecrated. The same is true of the element *-ja*, associated above with the dative or locative case of the third person enclitic pronoun.

Three inscriptions on libation tables—two from Palaikastro and one from Iouktas—continue the introductory formula as follows:

PK Za 11	*a-ta-i-301-wa-ẹ a-di-ki-te-te [*
PK Za 12	*a-ta-i-301-wa-ja a-di-ki-te[te*
IO Za 2	*a-ta-i-301-wa-ja ja-di-ki-tu ja-sa-sa-ra[me* etc.

Considering that the Minoan *a/ja* alternation is, as we saw, most probably of phonetic character, we have good reason for seeing all the sequences of the type *a-/ja-di-ki-te-te* (*ja-di-ki-tu* at Iouktas) as semantically identical. This allows us to isolate an additional formula characteristic of the inscriptions on libation vessels.

The fact that the beginning of IO Za 2 as adduced above is followed by the formula *u-na-ka-na-si i-pi-na-ma si-ru-te*—which, together with its variants, is attested twelve times in Minoan,[23]—makes it practically certain that *a-ta-i-301-wa-ja ja-di-ki-tu ja-sa-sa-ra[me* is a self-contained sentence. As far as I can see, the most plausible candidate for the role of the predicate in this formula would be the sequence *a-di/ja-di*. Note indeed that Minoan *a-di/ja-di* not only possesses the Anatolian third person singular present/future verbal ending, but is practically indistinguishable from the third person present/future of the Anatolian verb *a*(*ja*) (to make); compare Cuneiform Luwian *ati*, Hieroglyphic Luwian *á-ja-ti-i*, Lycian *adi/edi* (he/she makes/will make).[24] It seems to be best accounted for as standing for the future rather than for the present tense (there is no distinction between these two tenses in the Anatolian languages); accordingly, the sentence as a whole can be interpreted as a conditional clause of the type *if someone makes anything*.

The grammatical forms normally occurring in such prohibitive conditional clauses are relative and indefinite pronouns. Now in all IE languages, including the Anatolian, the relative, the indefinite, and the interrogative pronoun tend to be produced from two mutually related stems, *kʷ e- / *kʷ o- and *kʷ ei- / kʷ i. The form *ki-* would be a normal development of the latter stem, which is directly attested or safely postulated for a wide range of languages, including Indo-Iranian and Slavic languages, Greek, and Lycian.[25] The pronouns that are regularly formed on the stem in question are *kʷ is for the common gender and *kʷ i-t/d for the neuter (see Hit. and Palaic *kuis*, *kuit*, Av. *čiš*, *čiṭ*, Lat. *quis*, *quid*). These facts seem to account for the Minoan *kite-* (*kitu*), allowing tentatively to identify it as nom.-acc. sgl. neuter of the pronoun in question. As for the form *kitete*, it seems reasonable to take the second *-te* as a particle turning the relative pronoun into the indefinite one: although no direct Anatolian parallels are attested, both the structure of this form and some analogous formations in other IE languages fairly justify such interpretation.[26]

The above interpretation of *ki-te-te / ki-tu* as nom.-acc. sgl. neuter of the indefinite pronoun prevents us from taking it as the subject of *a/jadi*. This seems to leave our sentence (and, by analogy, other *a-di-ki-te-te* sentences as well) without grammatical subject. A similar situation can be shown to have existed in Lycian, which could form its indefinite prohibitive sentences both with and without the indefinite pronoun as grammatical subject. Consider, for example, such typical Lycian constructions, containing prohibition of additional burial in a given tomb, as *s]e [ije] n[e hrppi tâ]ti tice cbi hrpije mei tadi tice*, etc., (and [one] shall not put anyone else upon them; if [one] puts anyone upon them) with no grammatical subject in either if its two sentences. Applying this model to the Minoan *a-di-ki-te-te* sentences, we can suggest the following readings:

PK Za 11, PK Za 12:

a-ta-i-301-wa-ja	*a-di*	*ki-te-te*
(transposition):		
to/in *ata-*, and/but-thus-to/in it	if (one) makes	anything

(translation):
and/but thus if one makes anything to/in *ata-*, to/in it . . .

IO Za 2:

a-ta-i-301-wa-ja	*a-di*	*ki-tu*	*ja-sa-sa-ra-me*
(transposition):			
to/in *ata-*,	if (one) makes any		*ja-sa-sa-ra-me*

(translation):
and/but thus if one makes any *jasasarame* to/in *ata-*, to/in it . . .[27]

Prohibitive conditional propositions of this nature are abundantly attested in ancient epigraphic sources in both the Aegean and the Near East.[28]

Proceeding from the identification of the Minoan *a-di* as the third person present/future of the Anatolian verb *to make*, which I first suggested in an article published in *Minos*, Alexander Uchitel proposed to read one of the most common headings of the Hagia Triada archives, the sequence *a-du* (HT 85a, 86a, 88, 92, 95b, 99a, 133) as the third person imperative of this verb (cf. Cuneiform Luwian *a-a-du*). Its plural form, which should be phonetically rendered as *andu*, would give the meaning *let them do*.[29] Uchitel's identification of Minoan *a-du* as the third person imperative of Anatolian *a(ja)-* throws light on another heading, *je-di*, which occurs four times in the Hagia Triada corpus.[30] In a joint publication, I suggested that this heading should be interpreted as the third person singular/plural of the present/future of the verb *a(ja)*, that is, as meaning *they do/are doing* (Uchitel and Finkelberg 1995: 32). Following the same approach, we may identify the heading *a-da*, attested on a Tylissos tablet (TY 3a), as the singular/plural of the third person preterite of the verb *to make* (cf. Cuneiform Luwian *a-a-ta*, Hieroglyphic Luwian *á-tà*). We are therefore in the position to produce the following paradigm:

ind. pres./fut. 3rd pers. sgl./pl/	*a-di/ja-di > je-di*
ind. pret. 3rd pers. sgl./pl.	*a-da*
imper. 3rd pers. sgl./pl.	*a-du.*

Note that this paradigm is both impeccably Anatolian in its morphology and fits in perfectly well with the context of the archives.

Furthermore, in his "Records of Conscription, Taxation and Monthly Rations in Linear A Archives" Uchitel suggested that the sequence *a-ra-u-da* found in the heading of a Khania tablet (KH 5) should be interpreted as parallel to the third person preterite of the Hittite, Luwian, and Lycian verb derived from the stem *a-ra-wa-* (free) and should be translated *he released.*[31] This interpretation renders the heading of KH 5 exactly parallel to the Linear B formula PN/title *e-re-u-te-ro-se* SA x (PN/title released [from taxation] x units of flax) found in the Pylian records of flax delivered as tax (PY Na 395, 568, 924) and makes sense of the document as a whole.[32] If correct, the correlation *a-ra-u-da* = *e-re-u-te-ro-se* will give us the first full scale Linear A–Linear B parallel. Another important fact about Uchitel's discoveries is that they are exclusively based on the materials of the Hagia Triada and Khania archives, thus showing us that the results obtained in the course of the analysis of Minoan inscriptions on libation vessels can be applied with profit to other Linear A sources as well.

The Morphological Profile of Minoan

If the above interpretation makes sense, we can now attempt to draw what might be called the morphological profile of Minoan. With the assistance of such a profile, we can hope to define the place of the language of Linear A within the Anatolian family with greater precision. The following list of the morphological features of Minoan seems to be wide and diverse enough to furnish a solid basis for comparing it with the extant languages of the Anatolian group:[33]

(1) the 3rd person present/future verbal ending *-di* (*a-di/ja-di/je-di* [he/she/they make(s)/will make]);
(2) the 3rd person preterite verbal ending *-da* (*a-da* [he/she made], *a-ra-u-da* [he/she released]);
(3) the 3rd person imperative verbal ending *-du* (*a-du* [let him/her/them do, make]);
(4) the dative/locative case ending *-i-* (*a-ta-i-*, *ta-na-i-*,]*a-na-ti-*);
(5) (*i*)*ja* as the dative/locative case of the 3rd person enclitic pronoun (*-301-wa-ja*, *i-ja* at KN Za 10; CR[?] Zf 1);
(6) *-nu* as the accusative case of the 3rd person enclitic pronoun (*-301-u-ti-nu*, *u-ti-nu*,]*ti-nu*);
(7) the reflexive pronoun *-ti-* (*-301-ti*, *-301-u-ti-nu*, *u-ti-nu*,]*ti-nu*);

(8) - A 301 as the introductory conjunction *-and* or *-but* (*-301-wa-ja*, *-301-ti*, *-301-u-ti-nu*, *-301-*);
(9) the particle *-wa-* (or *-u-*), (*-301-wa-ja*, *-301-u-ti-nu*, *u-ti-nu*);
(10) *-wa-* as the introductory particle (rather than the particle of quoted speech);
(11) the fixed order of the introductory particles and pronouns:
    (a) the conjunction *and* or *but*; (b) the introductory particle *-wa-*; (c) the reflexive pronoun *-ti-*; (d) personal pronouns;
(12) the location of the chain of particles and enclitic pronouns after the first semantic unit of the sentence opening the discourse.

Finally, there are also two lexical elements whose identification seems to be solid enough to justify their being added to the list:

(13) the stem *a-* of the verb *to make* (*a-dilja-dilje-di*, *a-da*, *a-du*);
(14) the stem *arawa-*, free (*a-ra-u-da*).

The morphological features that the language of Linear A shares with the Bronze Age Anatolian languages seem to suffice for identifying it as a language of the Anatolian family. Thus, the third person preterite and imperative verbal endings, the dative/locative case ending, the accusative in *-n* of the third person enclitic pronoun, the introductory conjunction that opens the chain of particles, the particle *-wa-*, and probably also the stem *arawa* are paralleled in all the three Bronze Age Anatolian languages; *-di* as the third person present/future verbal ending and the reflexive pronoun *-ti-* are paralleled in Palaic and Luwian; whereas *-wa-* as the introductory particle, the order of the particles and enclitic pronouns, and the stem *a-* of the verb *to make* are paralleled in Luwian. We saw that the phonology of the language of Linear A does not contradict this conclusion. It can therefore be inferred with a considerable degree of certainty that the language of Linear A is an Anatolian language. Its identification with one of the extant Anatolian languages is, however, an entirely different matter. Correlation of the characteristic features of Minoan with the parallel features in other Anatolian languages, the results of which can be seen in figure 5, shows that Hittite disagrees with Minoan on seven points,[34] Palaic differs on at least three,[35] and Luwian disagrees with Minoan on two out of the fourteen points on the list. The two points in question are (*i*)*ja* as the dative case of the third person enclitic pronoun—the very feature that, as we saw, disqualifies Palmer's Luwian interpretation of KN Za 10—and the location of the chain of enclitic particles and pronouns.[36]

Let us now suppose, for argument's sake, that the location of the chain of particles is too trivial a distinction, whereas (*i*)*ja* should be discarded as due to erroneous identification. Does this bring the language of Linear A any closer to Luwian? The answer is definitely no. As we have seen, the identification of Minoan with

Fig. 5. The Morphological Profile of Minoan

	Hittite	Palaic	Luwian	Lydian	Lycian
(1)	negative	positive	positive	positive	positive
(2)	positive	positive	positive	negative	positive
(3)	positive	positive	positive	negative	positive
(4)	positive	positive	positive	negative	positive
(5)	negative	negative	negative	negative	positive
(6)	positive	positive	positive	positive	positive
(7)	negative	positive	positive	positive	positive
(8)	positive	positive	positive	positive	positive
(9)	positive	positive	positive	negative	positive
(10)	negative	negative	positive	negative	positive
(11)	negative	?	positive	positive	positive
(12)	negative	negative	negative	negative	positive
(13)	negative	?	positive	negative	positive
(14)	positive	?	positive	?	positive

Luwian is also impossible for phonological reasons. Namely, the distinctive feature of Luwian, which sets this language apart from the other languages of the Anatolian family, is the absence of the phoneme *e*, which is abundantly represented in Minoan.[37] The language we are looking for is, however, the one that would coincide with Minoan both morphologically and phonologically.

It thus emerges that the language of the Minoan inscriptions is an IE language of the Anatolian family whose phonetics goes with Hittite and Palaic and whose morphology goes with Luwian. No language possessing such characteristics is attested for Bronze Age Anatolia. Yet, the Bronze Age Anatolian languages are not the only languages of this family known to us. Several languages of the Classical Age, such as Lydian, Lycian, and Carian, have also proved to belong to the Anatolian group, so that our analysis cannot be complete unless it takes into account those languages as well. While the material on Carian is still too meager for an analysis of this kind, the evidence supplied by Lydian and Lycian seems to be sufficient for our purpose. When dealing with these two languages, it is important to keep in mind that we must take into account the phonetic developments that necessarily should have taken place during the millennium separating them from the Bronze Age Anatolian languages, developments that could well have modified the inherited forms in many and various ways.

Comparison with Lydian shows that it differs from Minoan on at least eight points (see fig. 5). As far as I can see, there is no way to account for these differences in terms of historical phonetics. This will suffice to conclude that the language of Linear A cannot count as a remote ancestor of Lydian. In contrast, the comparison with Lycian shows a high degree of correspondence with the distinctive features of the Minoan language. The agreement between the two languages necessitates reexamination of the entire ensemble of the morphological features of Minoan as against the corresponding features in Lycian. Note that nos. 2, 5, and 9 below can be consistently accounted for in terms of the well attested correspondence of Luwian *a* to Lycian *e* (cf. Luwian *tati* [father] as against Lycian *tedi*):[38]

(1) both Minoan and Lycian have *-di* as the third person present-future verbal ending;
(2) the Lycian third person preterite verbal ending *-dê* can be consistently accounted for as corresponding to the Minoan *-da* (see above, with n. 38);
(3) both Minoan and Lycian have *-du* as the third person imperative verbal ending;
(4) both Minoan and Lycian have *-i* as the dative case ending;
(5) the Lycian *ije*, the dative/locative case of the third person pronoun, can consistently be accounted for as corresponding to the Minoan *ija* (see above, with n. 38);
(6) both Minoan and Lycian have the accusative in *n* of the third person enclitic pronoun;
(7) both Minoan and Lycian have *ti* as the reflexive pronoun;
(8) both Minoan and Lycian have the introductory conjunctions *-and/-but*;[39]
(9) the Lycian particle *-we*, adjoined to the conjunctions *se* (and) and *me* (but) and the particle *uwe* adjoined to the same conjunctions, can be consistently accounted for as corresponding to the Minoan particle *-wa-* (see above, with n. 38);[40]
(10) the Lycian *-we* and *uwe* are used in the same way as Minoan and Hieroglyphic Luwian *-wa-*, that is, as particles introducing new sentences rather than particles of quoted speech;[41]
(11) the chains of particles in Lycian, though used less consistently than in the Bronze Age Anatolian languages, still display many characteristics of the ancient usage;[42]
(12) the location of the chains of particles in Lycian, viz., after the first semantic unit of the sentence opening the discourse, is strictly parallel to its location in Minoan;
(13) both Minoan and Lycian have *a-* as the stem of the verb *to make*;
(14) both Minoan and Lycian possess the stem *arawa-* (free).

In addition, the stem *ti-* of the Lycian relative and indefinite pronoun is the only Anatolian pronoun of this category that can be accounted for as a normal development of the stem *ki-*, the same one that was tentatively reconstructed above for the Minoan language (see above, with nn. 25 and 26).

As far as I can see, there is no disagreement between Lycian and Minoan that does not lend itself to explanation in terms of historical phonetics. In addition, the phonological feature that precludes identification of the language of Linear A with Luwian, namely, the phoneme *e*, offers no difficulty as far as Lycian is concerned. Although there are indeed cases in which Lycian *e* can be shown to correspond to Luwian *a* (cf. again Luwian *tati* [father] as against Lycian *tedi*), this development cannot account for all the occurrences of *e* in Lycian because, as was shown by Pedersen and, recently, by Melchert, as distinct from Luwian Lycian does possess the phoneme *e* (Pedersen 1945: 19, 33, 54–55; Melchert 1994: 311). Finally, it was noticed long ago that Lycian is inconsistent in rendering the *r/l* distinction. See, for example, such doublets as Lycian *Pinale* / Greek *Pinara* or the alternation *atrâ-* (acc.) / *atli-* (dat.) (person). It is obvious that this feature of Lycian goes well with the lack of the *r/l* distinction in the Linear A script.[43]

There is thus a high degree of correspondence between the phonological and morphological system of Minoan and that of Lycian. In view of this, there seems reason to conclude that the language of Linear A is either the direct ancestor of Lycian or a closely related idiom.

General Considerations

In Book 7 of his *History*, Herodotus wrote about the Lycians:

> The Lycians are in good truth anciently from Crete; which island, in former days, was wholly peopled with barbarians. A quarrel arising there between the two sons of Europa, Sarpedon and Minos, as to which of them should be king, Minos, whose party prevailed, drove Sarpedon and his followers into banishment. The exiles sailed to Asia, and landed on the Milyan territory. Milyas was the ancient name of the country now inhabited by the Lycians; the Milyae of the present days were, in those times, called Solymi. So long as Sarpedon reigned, his followers kept the name which they brought with them from Crete, and were called Termilae, as the Lycians still are by those who live in their neighbourhood. But after Lycus, the son of Pandion, banished from Athens by his brother Aegeus, had found a refuge with Sarpedon in the country of these Termilae, they came, in course of time, to be called from him Lycians.[44]

As emerges from the Lycian inscriptions, "Termilae" was indeed the name (actually, the only name) by which the Lycians called themselves; it is possible, then, that Herodotus' account of their migration from Crete originates from a genuine Lycian tradition.

As I have argued elsewhere, there is good reason to suppose that the linguistic substrate in the eastern Mediterranean should be identified as Anatolian or, more precisely, Luwian, and closely similar conlusions were made in articles published almost simultaneously by Onofrio Carruba and Robert Drews.[45] But although general cultural, historical, and linguistic considerations make it highly plausible that the language of Minoan Crete should be associated with Luwian,[46] we saw that the concrete linguistic evidence at our disposal renders such association impossible. It has been shown, however, first and foremost by Emmanuel Laroche, that Lycian is closely related to Luwian, so that Houwink ten Cate even coined the expression "West Luwian" to account for the position of this language.[47] If the interpretation proposed above is correct, the same definition would apply to the language of Linear A as well. This would mean, put as simply as possible, that the language spoken in Crete in the second millennium B.C. was part of the broadly defined Luwian idiom that spread over the entire territory of the eastern Mediterranean.

Notes

I am grateful to Craig Melchert for his constructive criticism of an earlier version of this paper, to Robert Drews for his invitation to take part in this Colloquium, and to Stuart Wheeler for his hospitality.

1. *ku-ka-da-ro* Uf 836; *ku-ku-da-ra* (HT 117 a7). For a representative list see Nagy 1963: 186–91.

2. The importance of this fact was already recognized in Evans 1909: 38.

3. The sign identified in *GORILA* as equivalent to Linear B no. 20 (*zo*) is a hapax in the Linear A script.

4. On the methodological importance of such internal demonstration see Barber 1974: 98 n. 26. According to Duhoux 1989: 72–74, the fact that many Linear B signs have no parallel in the Linear A syllabary indicates that the Linear B phonetic values cannot be applied to Linear A. Yet, Duhoux does not take into account the systematic nature of the difference in the inventory of signs dealt with in the two scripts or the fact that the Linear B operates with a much greater amount of signs than Linear A. It is noteworthy that in his later publications Duhoux does apply the Linear B phonetic values to Linear A texts. See. e.g., Duhoux 1994/95 and 1998.

5. Notwithstanding some acrimonious controversies with Gordon and his school, the main opponents addressed by Best are "Mycenological enemies of the decipherment of Linear A" (see Best and Woudhuizen 1989: 30). Yet, Best's own quotation of what C. J. Ruijgh, "one of the bitterest enemies of the decipherment of Linear A as a Semitic dialect," wrote in 1968 in connection with the Gordon hypothesis makes it clear that, rather than against the Semitic hypothesis as such, the Mycenologists' criticism is directed against the reliability of the etymological method used by its adherents.

6. KN Za 10: *ta-nu-a-ti ja-sa-sa-ra-ma / na da-wa . . . du-wa-na i-ja. . . .* See Palmer 1965: 333–38.

7. Cf. Ventris and Chadwick 1973: 395: "Thus the two series of dentals, which Linear B distinguishes as voiced and unvoiced, probably represented a different distinction in Linear A; it is Linear B *da* which corresponds to Cypriot *ta*, though *ti* and *to* agree in the two systems."

8. See Moscati et al. 1980: 46–49. Akkadian seems to be the only Semitic language that also possessed the vowel *e*. In the Akkadian writing-system, however, "the series of symbols with *e* is very incomplete" (47).

9. Craig Melchert, personal communication (Melchert's italics). See also Kammenhuber 1969a: 177–78; Carruba 1970: 40; Melchert 1994: 13–21, 38.

10. See, e.g., Melchert 1994: 40, 282–86 (Lycian), 43–44, 356–60 (Lydian).

11. See Friedrich 1969: 10–12; Laroche 1980, 22–23. As distinct from Hurrian, Elamite seems to possess no such distinction, but its system of vowels seems to have been based on *i*, *u*, *a* only. See Reiner 1969: 72.

12. KO Za 1, PK Za 12, TL Za 1, IO Za 2, IO Za 3, IO Za 7, SY Za 1, SY Za 2, SY Za 3, and probably also IO Za 4.

13. Cf., however, *-u-ti-nu* at IO Za 2 and *]ti-nu* at IO Za 11.

14. IO Za 8, IO Za 6,PS Za 2. See Finkelberg 1990/91: 48; cf. Duhoux 1998: 9–10.

15. Kammenhuber 1969a: 252: "Die heth.-luw. Sprachen erhalten ihr charakteristisches Gepräge durch *die Ketten von satzeinleitenden Partikeln, die jeweils an das erste Wort des Satzes gehängt werden*" (Kammenhuber's italics).

16. Although the Hittite reflexive pronoun *-za-* can occasionally be placed before the third person enclitic pronoun, the reverse order seems to be the norm in Hittite. See Friedrich 1974: 148; Carruba 1969: 39; Finkelberg 1990/91: 74 and n. 75.

17. The full spelling *ija* seems to be attested at KN Za 10, CR(?)Zf 1, and PR Za 1.

18. Minoan A 301 can be compared to Hittite *-al-ja* (and) and *-ma* (but) and to Luwian *-ha* (and) and *-pa* (but), all of them enclitic conjunctions attached to the first word in the sentence. Functionally, it can also be compared to the orthotonic Hittite *nu-* and Luwian *a-* (and) in that they also open the chain of particles.

19. The inscription on a pithos from Epano Zakros (ZA Zb 3) seems to be the only extant Minoan example of placing the introductory particles at the beginning of the second sentence (although the same may also be true of PS Za 2 and KN Zf 1).

20. Or, alternately, *santi kupapa waja* (Bossert's transcription). See Friedrich 1932: 146 and n. 1.

21. Cf. Barber 1974: "Since vocabulary is frequently borrowed wholesale, and since it is difficult or impossible to spot all borrowings on the first analysis, seeming correspondences both of vocabulary and of phonology (the sounds of borrowed words may correspond deceptively well) must be discarded as sufficient proof. Morphological systems which match in many fundamental details, however, furnish exactly the sort of evidence needed. Furthermore, the rarer or more anomalous the kind of system, the stronger the proof" (216).

22. The Hieroglyphic Luwian particle *-wa-* does not necessarily have the force of the particle of quoted speech. See Meriggi 1962: s.vv. *wa*, *a-wa-*, *-ha-wa*; Kammenhuber 1969: 172. Since the same seems to be true of the Minoan *-wa- / -u-*, I conventionally translate it as *thus* (Meriggi's translation of HLuwian *wa* is "dann," "ähnlich."

23. On this formula see Finkelberg 1990/91: 63–65.

24. CLuwian *ati*, HLuwian á-*ia-ti-i* and Lycian *adi* are opposed in this respect to Hittite *iiazi* and Lydian *int*. On the Hieroglyphic Luwian forms see Morpurgo Davies and Hawkins 1987: 276–79.

25. See Sanskrit *cit*, Avestan *čiš*, *čiṭ*., Slavic *čĭ-(to)*, Greek τίς, τί, Lycian *ti-(ke)*, all of them resulting from *$k^{w}i$- > *ki*, cf. Thessalian κίς and Mylian *ki* (who, which) (Mylian is a dialect of Lycian). On the Mylian form see Melchert 1993: 118.

26. Cf., e.g., the reduplication of *-to* in Slavic, which turns the relative *či-to* into the indefinite *či-to-to*, or the reduplication of the entire stem in Latin that turns the relative *quid* into the indefinite *quid-quid.* The reduplicated **kite-kite* (cf. Lycian *tise tise*) may perhaps have developed into *kite-te* as a result of syncope, but a parallel with the Hittite *kuitta* (whatever) and Lydian *qidad* is also possible.

27. On an interpretation of the Minoan *a-/ja-ta-* as *precinct* and *a-/ja-sa-sa-ra-me* as *altar* see Finkelberg 1990/91: 66–67.

28. A good Anatolian parallel is furnished by the Lycian formula *me-ij-adi tike tibe zummê* (but if someone makes here any damage whatsoever). *TL* 59, 2–3, cf. also *TL* 44, c 17; 91, 2–3; 95, 2–3. See also the alternative expression *adi mejê kttbâ tisñce* (*TL* 89, 2–3, cf. 90, 4).

29. Uchitel wrote in this connection: "Thus, the most common colophons of Sumerian economic texts, where they function as headings, are in fact various verbal forms: *šu ba-ti-* 'received', *zi-ga* - 'delivered', *mu-túm* - 'deposited'. Luwian hieroglyphic economic documents do not use either headings or colophons, but verbs specify every transaction: *piyamin(a)* - 'we give', *piyai* - 'he gives', *piya(n)ti* - 'they give', *usa(n)ti* - 'they bring.'" See Uchitel and Finkelberg 1995: 32.

30. HT 8a, 36, 122b, 140. Considering that the inscriptions on libation vessels precede the archives of Hagia Triada by no less than two hundred years, the difference between the *a-di/ja-di* of the former and the *je-di* of the latter can consistently be explained in terms of a phonetic development comparable to the alternation between *adi* and *edi*, the third person singular of the same verb, in Lycian.

31. On the Hittite adjectives *arawa-* and *arawan(n)i-*, and the verbs *arawahh-* and *arawes-*, see Laroche 1960: 124–28 and Friedrich 1979: 251–58; on *arawani-* (on the Kululu lead-strip 1) see Hawkins 1987: 143; on *arawa* in the Letoon trilingual see Laroche 1979: 1.12.21, p. 53. I am grateful to Yoram Cohen for drawing my attention to some of these publications.

32. Cf. Uchitel 1994/95: 78: "KH 5 appears to be a balance account of tax assessment, where the first part of the text says that somebody 'released' from taxation two units of *303 and two units of wine, while its second part . . . demands the payment of three units and some fraction of *303, and two units and some fraction of figs."

33. Since Minoan *ki-te-/ki-tu*, identified above as the relative/indefinite pronoun, is not paralleled in other Linear A contexts, I exclude this word from the list of the features that I see as essential to the subsequent interpretation.

34. In the dative case of the third person enclitic pronoun, in the stem of the reflexive pronoun, in the location of the chain of particles, and in the stem of the verb *to make* (nos. 5, 7, 12, 13); in addition, there is lack of correspondence in the third person present/future verbal ending, in the use of the particle *-wa-*, and in the respective positions of the reflexive pronoun and the third person enclitic pronoun within the introductory chain of particles (nos. 1, 10, 11). On the order of the Hittite particles and pronouns see n. 16 above.

35. In the dative/locative case of the third person enclitic pronoun, in the use of the particle *-wa-*, and in the location of the chain of particles (nos. 5, 10, and 12). There is no clear evidence as to the respective positions of the reflexive pronoun and the third person pronoun in the introductory chain of particles, and the Anatolian stems *a(j)a-* (to make) and *arawa-* (free) have not yet been attested in Palaic (nos. 11, 13, 14).

36. If the above identification of *ki-* as the stem of the relative/indefinite pronoun is correct, the same would also be true of this feature of the Minoan language. Note that it is

actually out of the question that Hittite, Palaic, and Luwian *kui-* can be reduced to *ki-* as a result of any phonetic development whatsoever. Although both forms derive from IE *$k^{w}i$- (see above with n. 25), Minoan *ki-* presupposes a loss of the labial component of the original labiovelar, a component which, centuries later, was still represented in Hittite, Palaic, and Luwian.

37. Note that since on the whole the Linear A texts predate any Cuneiform or moreover Hieroglyphic Luwian evidence, the phonological discrepancy between Minoan and Luwian cannot be explained away in terms of historical phonetics, that is, by taking Minoan *e* as a later development of Luwian *a*.

38. See Pedersen 1945: 19, 33, 54–55; Houwink ten Cate 1965: 197–98; Neumann 1969: 375.

39. Cf. Lycian *se* (and) and *me* (but). Cf. Houwink ten Cate 1965: 73: "From a syntactical viewpoint *me* can be compared with Hittite *-a/-ja* and Luwian *-ha;*" cf. also Carruba 1969: 76. Minoan -A 301 comes closer to Lycian *-me* in that it is also introduced at the beginning of the first semantic unit of the sentence.

40. See Lycian *sewe*, *mewe*, *me uwe*, *se uwe*, *se uwe ti*; cf. *ti uwe*, *hribe uwe*. Cf. Melchert 1993: s.v. *(u)we*; Finkelberg 1990/91: 78 and n. 83.

41. The particle *uwe* was identified as a Lycian equivalent of the Anatolian *-wa-* in Carruba 1969: 96–100. As to *-we*, the only thing that prevented identification of this particle with Luwian *-wa-* seems to be the fact that, as distinct from the Luwian particle, the Lycian *-we* does not introduce quoted speech, see Houwink ten Cate 1965: 75 n. 11. However, we saw that the particle *-wa-* does not introduce quoted speech in Hieroglyphic Luwian either (see above, n. 22). Its function in Lycian is, therefore, strictly parallel to its function in Minoan and Hieroglyphic Luwian.

42. On the Lycian particle chain see also Garrett 1992.

43. Note that in the *atrâ- / atli-* declension, *l* regularly emerges in the dative (*atli*, passim, *etli* TL 117 3) and *r* in the accusative case (*atrâ TL* 44b 43; *atru TL* 25a 4). It can be suggested in view of this that the Lycian *r/l* alternation at least partly depended on the "harmony of sounds" within the word, that is, that *r* emerged when the word ended in *a* or *u* (*Pinara, atrâ, atru*), and *l* emerged when the word ended in *i* or *e* (*Pinale*, *atli)*.

44. Hdt. 1.173. Cf. also 7.92: "These people [the Lycians] came from Crete, and were once called Termilae; they got the name which they now bear from Lycus, the son of Pandion, an Athenian" (*The Greek Historians*, tr. G. Rawlinson [New York: Random House, 1942]).

45. Carruba 1995: 18; Drews 1997: 163; Finkelberg 1997; cf. Renfrew 1998: 252.

46. See, e.g., Singer 2000: 25: "If indeed Cretan is a Luwian language, its use in Western Anatolia should not come as a surprise. . . . [T]he entire Aegean basin must be considered to have been a basically homogenous cultural *koine* already in the second millennium BCE."

47. Houwink ten Cate 1965: 72.

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RECONSTRUCTING PROTO-INDO-HITTITE

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When in 1994 I wrote "Indo-Hittite Revisited" (Lehrman 1996), the only accepted subgrouping of Indo-European languages was the schema in which Anatolian (Anat.) is a descendant of Proto-Indo-European, like Indo-Iranian, Greek, Celtic, Germanic, Tocharian, and the rest. The numerous oddities of Anatolian, which could not in good faith be reconciled with Proto-Indo-European—as reconstructed by the dominant scholars such as Watkins, Eichner, Schindler, Rix, and their followers (Jasanoff, Melchert, Oettinger, et al.)—were explained as Anatolian losses, if considered seriously at all. The Indo-Hittite hypothesis, advanced by Paul Kretschmer in the early 1920s and fully fledged by Edgar Sturtevant in the late 1920s and 1930s, was thoughtfully reformulated (with some concessions to the hostile majority) by Warren Cowgill in the 1970s (see Cowgill 1975, 1979, with lit.), but it was, predictably, ignored or rejected. Wolfgang Meid, in 1973 and subsequently, attempted to accomodate the undeniably ancient look of Anatolian and the comparatively innovative look of the non-Anatolian Indo-European by replacing the old genealogical tree schema with something like a fluvial one (see Meid 1975, 1990). In his hypothesis, the Anatolian rivulet left the Indo-European riverbed much earlier than all the other known Indo-European rivers and brooks, long before the mother river produced the numerous innovations. Meid's hypothesis had enjoyed some vogue—because it was not the "Indo-Hittite" family tree so odious, for various unscholarly reasons, to so many (because Sturtevant had not been a nice man; because it was hard, for some, to lose "-germanisch" in "Indogermanisch"; who knows what else)—until I managed to show, in the above-mentioned paper, that Meid's river, if rid of conceptual problems, was actually Sturtevant and Cowgill's Indo-Hittite family tree in disguise. The same is true of the Gamkrelidze-Ivanov "spatial and derivational" subgrouping model (based on August Schleicher's schema) in which Anatolian split off from the Proto-Indo-European unity before that unity broke up into the Tocharian-Italic-

Celtic subgroup, on the one hand, and, on the other, a subgroup much later becoming differentiated into the Aryan-Greek-Armenian subgroup and Balto-Slavic-Germanic subgroup (see Gamkrelidze and Ivanov 1995: 1:348ff). Essentially, the Gamkrelidze-Ivanov model implicitly makes the same claim as the Indo-Hittite theory, namely, that Proto-Anatolian (PA) separated from its parent language, whereupon it and the rest developed separately from one another for a long enough time to split up into subgroups; the rest, that is, the parent of the non-Anatolian IE, developed in close enough quarters for a long enough time to create numerous shared innovations not found in Anatolian, while Anatolian developed its own shared innovations not found in non-Anatolian IE. Or we can say in fact—and from the point of view of *language* history it is exactly the same—that it was the non-Anatolian PIE that separated from Proto-Indo-Hittite and hived off on its own, leaving the rest—Proto-Anatolian—to develop its common innovations and to split up into subgroups, of which Hittite and Luwian are the clear two. This latter seems to me by far the simplest scenario. Its likelihood is greatly increased by the fact that Proto-Anatolian remained in Anatolia, where its descendants subsequently withered, whereas the folk that left their Anatolian homeland proceeded to develop an innovative form of speech known as Proto-Indo-European, the descendants of which spread far and wide.

What I find in 2000 is that many scholars have accepted the Indo-Hittite theory in some form—either in name as well as in essence, like Don Ringe (e.g., Ringe 1998) and Anna Morpurgo Davies,[1] or in essence if not in name, as Craig Melchert has done.[2] I am certainly heartened by this quick change in perspective. I see the IH theory as capable of curing some of the grave ills in the current reconstruction of PIE. These ills—phantoms and anachronisms—are sorely in need of exorcism or correction, depending on whether one is dealing with phantom sounds, such as the "laryngeals" in Proto-Indo-European posited for purely structural, that is, cosmetic reasons, just because some scholars, in line with their structuralist and generativist training, prize economy and symmetry above all else,[3] or whether one is dealing with roots and suffixes which these scholars freely slice apart or splice together because, in line with the nominalist worldview instilled by the same structuralist and generativist training, they see reconstruction as little more than a collection of mnemonic, or virtual, formulae rather than as an honest-to-goodness attempt to reconstruct, with all due caution and humility, actual words of a prehistoric language.

Yet another lapse of clarity, responsible for the false subgrouping of Anatolian and Indo-European, is the watering down of the shared-innovations principle, the only valid criterion for determining how copies of a phenomenon are related to each other and to the phenomenon in question. This lapse, caused by the structuralist and generativist overprizing of the systemic in language, happens to be, to this day, a basic tenet of the Harvard school and of those in Europe (such as Wolfgang Meid and Werner Winter) who are closely allied with it. Calvert Watkins has stated on numerous occasions that he does not put much stock in shared innovations in determining language subgrouping. We have the earliest published statement of this in Watkins's 1966 paper "Italo-Celtic Revisited" (Birnbaum and Puhvel 1966, 29 ff.):

> The common innovation is so firmly anchored in traditional linguistic thinking that to question its utility smacks of lèse majesté. But one may be permitted to suggest that it is overemphasized. For to consider common innovations alone in subgrouping, and to ignore common retentions, is possible only if we consider language as a mechanical sum of linguistic features, some of which "change" and some of which do not—the atomistic view of the neogrammarians. It is inconsistent with the concept of language as a structured system, and of linguistic history as a succession of systems. At any given stage of a given language, retentions and innovations are part of the same synchronic structure. If two languages agree in maintaining the productivity of a given structural feature, rather than replacing it by something else, we may well have in this retention a primary dialectal datum.

Clearly, no one examined this opinion in 1966, and with structuralism (and then its parricidal but remarkably similar descendant and rightful heir, the inherently ahistorical and meaning-hostile generative grammar) being the dominant ideology in North American language studies for several decades, the opinion spread far beyond its original precincts. The acceptance of structuralist tenets was too widespread and unconscious for anyone to argue that the unchanged preservation of inherited material merely shows the already established fact of genetic relationship, and fails to indicate anything whatsoever about the subgrouping of the languages that have retained the material. This, along with other components of the structuralist fallacy, among them the arbitrariness of the linguistic sign and the "synchrony/diachrony" dichotomy,[4] has remained part of the unexamined theoretical heritage of the Harvard school and its alumni to this day.

In line with this structuralist corrosion of the historical approach, the notion of shared innovations became replaced with the notion of the isogloss, borrowed from the arsenal of language geography. The Watkinsian isogloss may thus be a shared innovation or a shared retention. Yet subgrouping can be performed only on the basis of shared innovations, never on the basis of shared retentions. A shared retention, or archaism, is only a projection—or, more accurately, a retrojection—of a shared innovation. When Calvert Watkins says that a "number of archaic features in morphology and phonology set Anatolian apart from the other branches, and indicate that it was the earliest to hive off" (Watkins 1998: 31), he admits, clearly unbeknownst to himself, that the non-Anatolian branches share a number of innovations in morphology and phonology as against Anatolian, which is precisely the claim of the Indo-Hittite theory of subgrouping. The only argument Watkins can muster against the Indo-Hittite theory is that it "ha[s] not found a following" (Watkins

1998: 31)—an argument that is, typically, not scientific but sociopolitical: an idea, to him, is only as good as the number of adherents it has rallied.[5]

In 1508 Erasmus, very much against the consensus of his time, postulated that all the surviving manuscripts of the text of the New Testament had descended from a single archetype no longer extant and that had to be reconstructed. The consensus prevailed, however, for some two hundred years, until Johann Albrecht Bengel surmised, in the 1730s, that the "genealogic affinity" of one manuscript to another could be established on the basis of copying errors made by scribes while copying the original. Bengel's technique, in the classic form given it by Paul Maas, is nicely summed up in Reynolds and Wilson 1991 (211–12): "[T]hese errors can be divided into (*a*) those which show that two manuscripts are more closely related to each other than to a third manuscript (conjunctive errors), and (*b*) those which show that one manuscript is independent of another because the second contains an error or errors from which the first is free (separative errors). Care is taken to see that these errors are *'significant', i.e. not such mistakes as two scribes are likely to make independently, or such as a scribe could easily remove by conjecture* [italics mine]. On this basis the interrelationships of the various manuscripts and manuscript groups are worked out step by step until, ideally, a stemma of the whole manuscript tradition has been reconstructed." With these conceptual breakthroughs, achieved by textual critics, the critical and comparative study of language became possible.

It was the stemmatic "genealogical" model, establishing "families" of manuscripts, that made it possible for Sir William Jones, who had been schooled in the English philological tradition exemplified by Richard Bentley, to make his famous statement about the "affinity" of Sanskrit to various European languages. Jones did not give any specific examples in the published text of his much-quoted speech. The first person to have organized and presented his data in accordance with clearly described principles was the Danish scholar Rasmus Rask, who set up the precise criterion for establishing and describing the affinity:

> A language, however mixed it may be, belongs to the same family as another if it has the most essential, indispensable and primary words, the basis of the language, in common with it. . . . When in such words are found agreements between two languages, in such number that one can set up rules for the correspondences of letters between them, then there is a basic affinity between these languages, especially when the similarities in the languages' structures and inflection confirm that. (Rask 1818: 35–36, my translation)

This criterion has been refined somewhat in the last 186 years (by replacing "letters" with "sounds"), but it has not been essentially improved on. Building on the method and practice pioneered by Rask, we now say that *two languages are genetically related if and only if they show enough features in common, so that the likelihood of all these* *features having arisen independently in each language or having been borrowed—either by one language from the other, or by both languages from some third language—is minimal.*

The similarities most indicative of genetic affinity involve whole words (grammatical affixes and lexical roots taken together) which have identical or closely similar meanings in the languages being compared. The phonetic shapes of these words must be related to each other by means of *regular sound correspondences* and their environments.

The genetic relationship of two languages is established on the basis of their *shared inheritance* from a common ancestor. Yet it is only on the basis of *shared innovations* that we can determine the affinity of related languages more precisely by arranging them into *subgroups*. The members of a subgroup are more closely related to one another than any member of the subgroup is to a language outside the subgroup. The classic statement on language subgrouping belongs to Karl Brugmann (1884: 253):

> Es ist nicht eine einzelne und sind nicht einige wenige auf zweien oder mehreren Gebieten zugleich auftretende Spracherscheinungen, die den Beweis der näheren Gemeinschaft erbringen, sondern nur die große Masse von Übereinstimmungen in lautlichen, flexivischen, syntaktischen und lexikalischen Neuerungen, die große Masse, die den Gedanken an Zufall ausschließt.
>
> It is not a single language phenomenon nor just a few phenomena occurring simultaneously in two or more domains that constitute proof of a closer affinity, but only a large mass of agreements in phonetic, inflectional, syntactic, and lexical innovations that excludes the thought of a coincidence.

Two or more related languages are subgrouped together if they share a large number of innovations not found in other related languages—or, if they are, they can be shown to have arisen there independently. Recall in this connection the stemmata describing the descent of manuscripts: shared innovations would be the "conjunctive errors," and those shared by another group would be the "separative errors." "Innovation," then, is a more general term, capacious enough to subsume both scribal error in textual criticism, letter-shape change in paleography, alteration in a language form, and genetic mutation in biology. Note also that the term *significant* is frequently misunderstood by linguists: all it means is that any innovation that is likely to be *shared* and not made independently by the languages in question after their separation is *significant*, or relevant, for subgrouping.

A large number of shared innovations in two or more languages can only be explained if these languages, thus forming a separate subgroup, descend from a single common language, an ancestor of their own, which was distant enough in space and time from the ancestor of the entire family so that there would not have been any contact between them. If no such neat subgroups can be established, then the attested languages must be regarded as direct descendants of the family's protolanguage, and their diversity would then reflect divergent dialects of the protolanguage rather than an intermediate set of ancestor languages between the attested languages and the ancestor language of the entire family.

2

The attempts to replace the tree-like stemma with a more "fluid," Heraclitean account of language change attempted by Johannes Schmidt's "wave theory" and Wolfgang Meid's "river theory" miss the point: whether we image the events as "waves" of replacement or as a "flow" of stages, with dialect "outflows" at different points, the results are essentially the same, namely, *innovations occur and replace "archaisms,"* which, in turn, we recognize as such only thanks to the innovations. The woody old stemma describes the essence of the process best.

It is good to recall and recognize at this point another important factor that has also fallen into disuse of late—the absolute age of the attested witnesses. Its importance may be illustrated by an example from art history. A Benvenuto Cellini bowl, even if it looks "old" and may, in certain respects, be a meticulous copy of certain elements of first-century B.C. Hellenistic artifacts, is obviously not a genuine first-century B.C. artifact: Cellini changed various details of shape, design, and proportions to suit his own notions, as well as those of his clients. Clearly, Cellini's original model or models, now lost, is (are) authentic, while his copy, however "old" looking it may be, is still fifteen centuries younger and cannot be used to make inferences about first-century B.C. artifacts.

The same may be said of copies of a text whose original is lost. The older the copy, the more valuable it is as a witness, because the fewer years that intervene between the copy and the original, the fewer potential changes there are that cannot be accounted for. The Old Russian classic *Slovo o pŭlkou Igorevě* is known only from a very imperfect eighteenth-century publication of a vanished sixteenth-century copy of a twelfth-century original. While on the whole we may recognize that the publication does represent an old Russian text, we cannot use it as a reliable witness in most details of its language because its original shape is recoverable only through emendation to begin with.

The older the attested documents, the more valuable their language is, obviously, as a witness for the oldest state of the language in question. Among the non-Anatolian languages of our family, the oldest witnesses are Indo-Iranian and Greek: the first nine books of the Rigveda for the Indic and the Avesta for the Iranian, both reflecting the state of the language by 1000 B.C. Greek is represented in the Mycenaean texts dating from the fourteenth through the twelfth century B.C. and in the Homeric texts reflecting the state of post-Mycenaean Greek of c. 800 B.C.[6] The rest of the non-Anatolian witnesses are far less old and, for comparative purposes, their facts *must always be checked* against the evidence of the old Indo-Iranian and Greek. We must be particularly cautious with the material of the Tocharian languages, attested between the seventh and ninth centuries A.D., the Balto-Slavic languages, attested from the ninth century A.D. (Baltic even later, from the fifteenth century), and with that of Albanian (the earliest documents from the sixteenth century). It has become fashionable of late to showcase Tocharian as the next most "archaic" branch of Indo-European, allegedly second only to Anatolian in its "hiving off" from the "Old Indo-European" or something like that (see, e.g., Schmidt 1992; Winter 1997; Gamkrelidze and Ivanov 1995; Melchert 1998; Ringe 1998). Yet Tocharian, with some three millennia of its speakers' unreported wanderings and contacts separating its first attestation from Proto-Indo-European, must be disqualified hands down from any serious consideration as a rival to Anatolian, attested as early as the nineteenth century B.C., if we start counting—as well we should—from Akkadian records of Hittite proper names. Tocharian innovations in phonology and morphology testify to its prolonged contacts with some—probably Uralic—languages; its geographic position (Chinese Turkestan) supports these linguistic inferences.[7] This makes the heavily innovative Tocharian largely unusable even as far as the reconstruction of Proto-Indo-European goes, let alone the reconstruction of Proto-Indo-Hittite. Its archaizing position in Ringe's Indo-Hittite tree (e.g., Ringe 1998: 43) or in Melchert's Old Indo-European tree (Melchert 1998: 25) as well as in Gamkrelidze and Ivanov's Schleicherian model mentioned above is, to my mind, indefensible.[8]

3

The ultimate task of the language historian is the synthesis of all that he has learned through the analysis and critical investigation of the origins of individual forms, both lexical and grammatical, in the attested witnesses—a synthesis comprising the reconstruction of the prehistoric language. But before he can proceed with reconstruction, he needs to determine which witnesses are reliable and which are less so or not reliable at all. Among the Anatolian witnesses-the oldest in our family—Hittite is surely the most ancient: in use in Anatolia between the seventeenth and thirteenth centuries B.C., with the documented presence of Hittite speakers as early as the nineteenth century B.C., based on the names and borrowings in the Old Assyrian documents found at Kültepe. Hittite and the two Luwian dialects—Cuneiform Luwian, the texts in which are contemporary with the Late Hittite period, c. 1380–c. 1220 B. C., and Hieroglyphic Luwian, the texts in which come from a wide area of Asia Minor and Northern Syria and range in date from ca. 1700 B.C. or earlier to the late 700s B.C.—provide sufficient material for our work. We may base our reconstruction almost entirely on the evidence supplied by these three witnesses, far more reliable than the rest. In addition, Hieroglyphic Luwian is written in what has to be, for the want of evidence to the contrary, the only writing system specifically designed for an Anatolian language. This writing system is also very old: the earliest specimens of it go back to the eighteenth century B.C., and it is likely that the writing system had been around for quite a while previous to the attested seal legends. The rest of Anatolian—the older-generation Palaic and the younger-generation Milyan, Lycian, Lydian, and Carian—are so poorly understood and scant of material that they should mostly be left alone, except in some few instances where they clearly match (do not contradict) the things already well-known from Hittite and Luwian. Very rarely will Lycian add something useful, as with regard to the quality of obstruents written in a clearly and consistently used Greek-based alphabet (e.g., Lyc. *tideimi* [child] vs. CL *titaimiš* (id.), where Lycian clearly shows the initial voiceless *t-* and the medial voiced *-d-*, whereas the cuneiform is far less clear with its syllabary signs where the contrast is marked indirectly and rather inconsistently). It must be added here, to preclude any hubris on the part of an Anatolianist, that close to half of all Cuneiform Luwian words are either poorly understood or not understood at all, and much the same situation obtains in Hieroglyphic Luwian. So much for the relatively reliable witnesses. Hittite, by comparison, is understood almost perfectly, which accounts for its continuing privileged position as the best Anatolian witness.

Palaic, which seems to be contemporary with the Old Hittite texts, is even more imperfectly known from fragments of rituals preserved in the archives of Boğazköy. Within Anatolian, it seems to be closer to Hittite than to Luwian (i.e., it may be said to form a subgroup with Hittite), but very little in Palaic—about 15 percent at the most—can be of any use.

Milyan (or Lycian B), in which sides three and four of the Xanthos Stele and one other text are composed, Lycian (or Lycian A), Lydian, and Carian, all from the first millennium B.C. and written in epichoric alphabets descended from the Greek alphabet, are largely unusable for comparative purposes because of our insufficient understanding of them.

Lycian is known chiefly from tomb inscriptions and coins from Lycia, in the southwest of the Anatolian peninsula, which date from the fifth and fourth centuries B.C. Lycian is close to (seems to have enough features in common with) Cuneiform and Hieroglyphic Luwian but does not seem to be descended directly from either. The shorter, more trivial inscriptions are understood fairly well, yet still leave many gaps in our knowledge. Longer inscriptions, especially the Xanthos Stele, and the famous Létôon trilingual (Lycian with Greek and Aramaic summaries), are still very poorly understood despite the numerous efforts to unravel them. The recent attempts to use Lycian for positing Proto-Anatolian sound changes and categories—particularly Melchert's attempts to find the feminine gender in Lycian and to attribute it to Proto-Anatolian, as well as his attempt to derive Lycian *e* directly from PA *o[9]—are ill-conceived, to put it mildly: that which is not well understood should not be used to explain that which is better understood, nor should it be used to posit that which is not attested (e.g., the vowel *_o_) in the languages that are far better understood.

Lydian inscriptions, far fewer than Lycian, and written in an alphabet similar but not identical to Lycian, date mostly from the fourth century B.C., with a few going back to the fifth or even sixth century. The language is still very poorly understood. It is clearly of the Anatolian branch of the Indo-Hittite family, but it does not appear to belong to the Luwian-Lycian subgroup of Anatolian, and its relationship to the Hittite-Palaic subgroup is also not all that clear.

In the cases of Palaic, Milyan, Lycian, and Lydian, not to mention Carian, the only thing we can do with integrity at this point is suspend judgment: except for some few forms and several details, the material of these languages is mostly opaque.

Carian, in Southwest Asia Minor between Lydia and Lycia, is known from inscriptions dating from the seventh century B.C. and later. The writing system has not yet been deciphered with certainty. The language is clearly Anatolian, but so many uncertainties remain that it is premature to use this language for historical-comparative purposes.[10]

Only a few inscriptions survive of Pisidian and Sidetic. The less said at this point about the contribution these two can make to language comparison the better.

4

As for the nine major (non-Anatolian) Indo-European subgroups, each with its corresponding intermediary protolanguage—Indo-Iranian, Greek, Italic, Celtic, Germanic, Armenian, Tocharian, Balto-Slavic, and Albanian,—all of them taken together go back to a protolanguage that shows numerous significant phenomena not found in the Hittite-Luwian or Anatolian subgroup, along with some significant losses (or "negative innovations" in Watkins's term). The utter absence of any relics of these phenomena in Anatolian (such as the simple thematic presents and the feminine gender, pace the unconvincing attempts of some scholars to chip away at it) and a totally different structuring of certain grammatical phenomena (such as the Anatolian lexical aspectual contrasts and the "receptive voice," as I call it, surviving in the Hittite _hhi-_ conjugation,[11]) compared with the related yet totally restructured categories of non-Anatolian Indo-European, make it imperative to infer that the non-Anatolian Indo-European _innovated_ by creating them after it split away from the common protolanguage.

Among the shared innovations of Proto-Indo-European, then, are the following:

(1) The loss of the Proto-Indo-Hittite laryngeals *x, *γ and the loss of the obstruent in the labialized *x^w, *γ^w.(For Proto-Anatolian, where the contrast of "voice vs. voicelessness" was replaced, as far as I can tell, with "short obstruction vs. long obstruction" [= "single vs. geminate"], I write *_hh_ for *x and *_h_ for *γ.) E.g., PA *_hanna-_ > Hitt. _hanna-_ (grandmother) (for _-h-_,

cf. compound _hubha-hanna-_ [grandpa and grandma]), Lyc. _xñna-_ :: PIE *_ano-_ > Lat. _anus_ (old woman), OHG. _ana_ (grandmother), etc.; PA *_hant-_, (medially _-hhant-_ in _mena-hhanda_ [front]) > Hitt. _hanza_, C.Luw. _hantiš,_ Hitt. _hantezzi(ja)š_ (first), C.Luw. _hanteleš_ id., Lyc. _xñta-wata-_ (ruler), etc. :: PIE *_ant-_ in Gk. _ant-í_, Lat. _ante_, etc.; PA *_hawhha-_ (grandfather) > Hitt. _hubha-_ C.Luw. _huuha-_, Lyc. _xuga-_ :: PIE *_awo-_ in Lat. _avu-s_, etc.; PA *_h^{w}aj-_ _/*h^{w}i-_ (to run) > C.Luw. redupl. _hu(u)ul/i/e-hu(u)iia-_ (run, march); non-redupl. Hitt. (hhi-conj.) _huwaai_ 3 sg pres act, _huehhun_ 1 sg pret (<*_h^{w}aj-hhun_), 3 sg pret _huwaaiš_ (<*_h^{w}aj-s_), 3 pl. pret. _huwaair/_ 3 pl. pres. _huianzi_ [_hwianzi_], pple. _huian_, causative _huinu-_, etc.; C.Luw. _huuiia-_, caus. 1 sg pret. _huinuwahha_; H.Luw. _hwi/a-i(a)-_ id., redupl. suff. _i_-conj. stem (PES)-_hwi-hwi-sà-_ (run), march'; PA *_pahhw-r̥/*p(a)hh^{w}an-_ (fire) > Hitt. _pahhur/pahhuwar_, gen. sg. _pahhuenaš_ (_-e-_ analog. to umlauted loc. _pahhueni_, see below), C.Luw. _paahuur_ :: PIE: Gk. _pûr_, Go. _fon_ (gen. _funins_), ON _fúrr_, OHG _fiur_, Toch. B _puwar_, Arm. _hur_ (gen. _hroy_) (torch); PA *_t(a)rhh^{w}-_ (to overcome) > Hitt. _tar-uh-/ta-ru- uh-/tar-hu-/ta-ru-uh-h-_, _tarhueešš-_ (become victorious); _Tarhunt-_ (theonym); _tarhuili-_ (victorious), H.Luw. _Tarhuzasa_ [_Tarhhunts_ theonym], Lyc. _Trq(q)as/Trqqñt-_ :: PIE *_tr̥̄w-_, *_tr̥̄-_ in Ved., _tūrvasi_, _turute_, etc.

(2) The umlaut of the Proto-Indo-Hittite *_a_ to IE *_e_ and to IE *_o_ (cf. the umlaut of *_a_ to *_e_ in Hittite, with the Luwian subgroup preserving the original *_a_, except when it is raised to _i_ in the unaccented final syllable before *_s_ and in the immediate neighborhood of Anatolian *$k^{j(h)}$ [: IE *$g^{j(h)}$] which, in Luwian, subsequently disappears).

(3) The feminine gender. The endings *_-ā_ and *_-ī_ (the so-called "motion suffixes") deriving feminines from some of the inherited vocabulary (e.g., IE *_ék^{j}w-o-s_ [horse], in Ved. _áśvas_, in Lat. _equos_: "motioned" *_ék^{j}w-ā_ [mare], in Ved. _áśvā_, in Lat. _equa_; *_wĺ̥kw-o-s_ [wolf] : *_wl̥kw-ī́_ [she-wolf], e.g., Ved. _vŕ̥kas_: _vr̥k-ī́_[_-s_], ON. _ylgr_), but cf. the "unmotioned" feminines *_mātēr_ [mother], *_awsōs_ [dawn]; cf. the "motioned" forms in later-attested IE languages, e.g. Albanian _motrë_ [_motër_] [sister], Lith. _aušrà_ and even Lat. _aurór-a_). The feminine remains restricted, however, particularly in the adjectival formations where the inherited common-gender forms (the innovated "masculine") is used to modify both masculine and feminine nouns. (On the alleged "_ī_-motion" in Anatolian see below.)

(4) The development of the dual.

(5) The development of the *_so-_*_sā-_*_tod_ set of demonstrative pronouns.[12]

(6) The development of the perfect tense out of what I call the receptive voice characterized, in PIH, by the *a-vocalism of the root (corresponding to the *o-grade vocalism in PIE) and a special set of endings.

(7) The development of paradigmatic aspectual stems (punctual, or aorist, and durative).

(8) The development of the simple thematic class of imperfective verb stems (including an entirely new set of lexical items belonging to this class) and of the closely related subjunctive.
(9) The development of the optative (suffix **-jē-/*-ī-*).
(10) The development of active and passive participles for each aspect and for the perfect.

The "positive" innovations—items 2 through 10—obviously cannot be reconstructed for Proto-Indo-Hittite, the mother language of the two sisters, PIE and PA.

At this point I am going to offer some rough chronological estimates as to when the separation of dialects occurred. As I cannot, at this point, understand one of the basic premises of glottochronology,[13] I prefer to estimate the time lapsed between the full development of different languages out of a common language impressionistically, without any scientoid pretensions, and merely based on some clear precedents. The clear precedents which seem to show that this time is about eight hundred to a thousand years include Latin, the protolanguage of the Romance family, the expansion of which began in the third century B.C. and resulted in the differentiation of several separate Romance languages (i.e., separate bundles of significant innovations) by roughly the eighth century A.D.; they include Proto-Norse, the Runic texts, which are attested in the second or third century A.D., while the fully differentiated Norse languages—Norwegian-Icelandic, Danish, and Swedish—are attested from the eleventh to the thirteenth century; these clear precedents also include Old Russian, which we may call Proto-East-Slavic, which was still relatively unified by A.D. 1240 and is now, some 800 years later, represented by three separate languages (Russian, Ukrainian, and Belarusian) which are rapidly developing ever more significant innovations that will increase their mutual unintelligibility in the centuries to come.

Using this, admittedly impressionistic, method of estimation and counting back roundly in thousands from the year 1000 B.C., when both the Anatolian languages and the non-Anatolian Indo-European ones had already been fully differentiated and spread over most of their present territory, I arrive at 2000 B.C. as the time when Hittite and Luwian had been well established and substantially differentiated for some time, and project another thousand years back to 3000 B.C., which must have been the point when the differentiation of Anatolian into Hittite and Luwian had begun: before that Proto-Anatolian was still a single language. Four thousand B.C. was roughly the year when the split between PA and PIE had occurred, which brings us, another millennium back, to 5000 B.C., at which point PIH must have been already sufficiently differentiated from Proto-Indo-Uralo-Altaic, or some such proto-entity, about which I will remain silent at this time.

After the future PIE tribes had left, then, around 4000 B.C., the remaining community did not sit still. It proceeded to embark on new projects, respond to and create new challenges, and thus develop new speech habits. It remained relatively coherent long enough, however, to develop, by ca. 3000 B.C., a few significant shared innovations. These shared innovations of Proto-Anatolian include the following:

(1) The replacement of the contrast of voice vs. voicelessness among the obstruents with the time of release ("length") contrast, i.e., plain vs. geminate.
(2) The loss of the obstruent "length" contrast in the word-initial position. (That this is an Anatolian loss rather than an IE innovation is made far more likely by the fact that the contrast "voiced vs. voiceless," which clearly exists in PIE, would be unrecoverable and impossible to explain. The same argument applies to the laryngeals and whether they constitute an IE loss or an Anatolian innovation. Contrary to Don Ringe's claim—"It follows automatically that nothing can be reconstructed for Proto-Indo-Hittite (PIH) unless it is attested in Anatolian and at least one other branch" (Ringe 1998, p. 32),—we don't need the evidence of any "third language" to determine that. If a phenomenon, e.g., a laryngeal, occurs in some words but not in others, in some contexts but not in others, so that its occurrence is not predictable, we have to infer that, in all likelihood, it was there to begin with and we simply have to accept it.
(3) The loss of the velar element of the initial IH *g^{w}-: (cf. its retention in the continuation of the initial IH *g^{wh}- and in *k^{w} in all positions). This agrees with the neutralization of the "plain vs. geminate" contrast and shows that the "aspirate vs. non-aspirate" contrast was maintained. The clear example is provided by Hitt. *úemizzi* (finds): HL *wamiti* id. < IH *g^{w}*am-ja-ti* (if the Hittite were from **aw* + ***emjeti*, HL would have had ***awamiti* instead of *wamiti*, cf. HL *awiti* [comes]: Hitt. *úezzi*), Hitt. *walh-* (strike) < IH *g^{w}*l̥x-* (full grade IH *g^{w}*alx-* > IE *g^{w}*el-* > OE *cwelan*, Lith. *gélti* [ache, hurt, etc.]), Hitt. *warhui-* (raw, unkempt, brute) < IH *g^{w}*r̥*H^{w}*-i-* > IE *g^{w}*r̥-u-* (> Ved. *guru-*, etc.). The alleged Hittite words for *woman* (Neu's *kwinna-* and Güterbock's *kuwanša-*[see Güterbock 1992]) are mirages; CL has *waana-* (woman) (when the Hitt. is found it should look something like **wana-*); that Lydian *kāna-* means *wife* is anything but certain.
(4) The unconditional raising of the long **ā* to **ǣ* or **ē* (cf. Arabic).
(5) A remodeling of the receptive voice to the formation best known in Hittite as the *hhi*-conjugation.

The two better known subordinate branches of Proto-Anatolian, Hittite and Proto-Luwian, show quite a few significant innovations characteristic of their respective dialect groups. In 2000 B.C., then, we have two different political and linguistic traditions: the rapidly obsolescent Hittite, with its cuneiform, and the vigorous Luwian, using both the Akkadian-Hurrian cuneiform and its own autochthonous "hieroglyphic" writing system.

Among the innovations of Hittite are:

(1) The umlaut of PA *_a_ to _e_ or _i_, under conditions similar to but different from the umlaut of *_a_ to *_e_ (cf. also the IE umlaut of *_a_ to *_o_) in IE. Note that Luwian kept the three short vowels *_a_, *_i_, and *_u_ (matched by the long *_ā_—which probably sounded more like *_ē_—*_ī_ and *_ū_).[14]

(2) The umlaut of *_a_ to _u_ before *_m_ (in _a-pu-u-un_ acc. sg. c. < *_ab_^{h}_ám_, in _ku-u-un_ acc. sg. c. < *_k_^{j}_ám_, in +_u-un_ < *_-ám_ 1 sg. prt. active).

(3) The assimilation of Anat. *_tt_ (: IE *_t_) > _z_ [_ts_] and of Anat. *_t_ (: PIE *_d_) > _s_—but not of *(_t_)_t_^{h} (: PIE *_t_^{h}, *_d_^{h})—before *_i_ and *_j_, e.g., Hitt. _tuzziš_ (army) < Anat. *_tautt-i_(_ja_)_-s_ (cf. Osc. _touto_, Lith. _taut-à_ (folk), G. *_teut-ō_, Hitt. _-az_ abl. < Anat. *_-atti_ : Luw. _-ati_ (lenited), Hitt. _adanzi_ (they eat): Luw. _atanti_ < Anat. *_at-antt-i_, Hitt. _šiuš_ (sky-god): Luw., Pal. _tiwat-_ (= Hitt. _šiwatt-_ (day) < Anat. *_tiwatt-_) < *_tjēws_, *_tiw-_ (: IE *_djēws_, *_diw-_), Anat. *_i-t_^{h}_i_ (go!) imp.sg.2 > Hitt. iit (: IE *_i-d_^{h}_i_), Anat. *_h_^{w}_att_^{h}_-j-a-r_ (pulls) > Hitt. _huittija_, Anat. *_t_^{h}_i-ja-nti_ > Hitt. _tijanzi_ (they place: IE root *_d_^{h}_ē-_/*_d_^{h}[_ə_]-).

(4) The unconditional change of PA *_kk_^{j} (: PIE *_g_^{j}) to _k_ and *_kk_^{j} (IE *_k_^{j}) to _kk_: Anat. *_k_^{j}_ēr_/*_k_^{j}_r̥t-_ (heart) > Hitt. _kir_/_kart-_, Luw. _zarz_(_a_) (: IE *_k_^{j}_r̥d-_), Anat. *_k_^{j}_anu-_ (knee) > Hitt. genu- (: IE *_g_^{j}_énu-_).

(5) The remodeling of verb endings privileging the _mi_-conjugation over the _hhi_-conjugation (e.g., the replacement of the _hhi-_ 3 pl. prs. ending *_-ar_ with the _mi_-ending *_-anti_, probably due to the PA disappearance of the final *_-r_; the extension of the *_-i_, _hic et nunc_, marker to the _hhi_-conjugation (but later than the i-umlaut); the extension of +_u-un_—the Hittite continuation of the active 1 sg. prt. *_-ám_—to the preterite of the _hhi_-conjugation resulting in the ending _-hhun_).

Among the Luwian innovations are the following:

(1) The raising of Anat. *_-a-_ in the final closed unaccented syllable before *_s_ to the sound written as _-i-_ (mistaken by Oettinger 1987 and his followers for the "_ī_-motion").

(2) The loss of PA *_k_ (: IE *_g_) and the obstruent in *_k_^{j} (: IE *_g_^{j}) and *_k_^{w} (: IE *_g_^{w}), *_k_^{wh} (: IE *_g_^{wh}): Anat. *_t_^{h}_ukattar-_ (daughter) > HL _tuwatara_/_i-_, Lyc. _kbatr-_ (: IE *_dhug_(_ə_)_ter-_), Anat. *_k_^{jh}_asr-_ (hand) > Hitt. _keššar_/_kišr-_, CL _iššari-_, HL _is_(_a_)_t_(_a_)_ri-_, Lyc. _izre-_ (: IE *_g_^{jh}_esr-_).

(3) The lenition of intervocalic obstruents, including the lenition of intervocalic stops, weakening of the fricative, and the HL rhotacism (as part of the lenition, cf. the lenition of the American English intervocalic _-t-_ to the flap [_ɾ_] under certain conditions).

(4) The affrication of Anat. *_k_^{j} to _z_ [_c_] (cf. Indo-Aryan *_k_^{j} > _s_^{j}).[15]

(5) The remodeling of noun stems based on the raising of Anat. *_-a-_ to _-i-_ before _-s_.

(6) The remodeling of common-gender nominative pl. based on the accusative plus the pronominal pl. *-i* < Anat. **-aj* (Luw. acc. *-nz* [> Lyc. *-s*] < Anat. **-ns*, nom. pl. *-nzi*).

(7) The remodeling of the active and receptive voices privileging the active voice, with remnants of the receptive-voice forms (3 sg. prs.) and the ending **-xa* generalized for the preterite of both active and receptive.

5

Now to spell out what we can reconstruct for Proto-Indo-Hittite. Proto-Indo-Hittite had three short vowels—**a*, **i*, **u*—and apparently three corresponding long ones. It had most likely four series of stops—plain (**p *t *k*), plain aspirated (*p^h *t^h *k^h), voiced (**b *d *g*), and voiced aspirated (*b^h *d^h *g^h), just the ones reconstructed by Brugmann for PIE, and, as Brugmann saw quite clearly, not all of them enjoyed equal frequency of distribution: the plain aspirates were rarer than the rest, and **b* was very rare. Languages do have sounds that are rarer than others. Take English [*ž*] for instance: rarity, too, is a rate of *existence*. (Note also that there is no compelling reason—certainly not typology!—to revise this basic system, to say nothing of accomodating the developments of the much later branches, such as Germanic and Armenian, which may be explained by their immediate environments.) The velar stops included dorsal (**k*, etc.), palatal (*k^j, etc.), and labial (*k^w, etc.). There were probably four "laryngeals": **x *γ* *x^w *$γ^w$. There were only two genders (common and neuter), two numbers (sg. and pl.), probably five cases (nominative, accusative, genitive, dative/directional, ablative, and instrumental), three voices with their distinctive sets of endings (active, middle, and receptive, the latter two closely related yet different), two moods (indicative and imperative), two tenses (non-past and past), and a lot of stem-forming suffixes for nouns (such as **-a-*, **-r̥/*-n-*, **-wr̥/*-un-*, **-ant-*, **-[i]ja-*) and verbs (e.g., *-sk^ja-, **-ja-*, **-n-*).

Now we can, on the basis of the items reconstructed for both PA and PIE, sketch out a reconstruction of the vocabulary of PIH. This reconstruction amounts to a thorough revision of what has been done so far in reconstructing the lexicon of our family's protolanguage. The difference between a list of items reconstructed by comparing PA and the extra-Anatolian PIE on the one hand and the attempts using the majority subgrouping on the other is a difference of temporal depth: thanks to the PIH subgrouping schema, the reconstructed items consitute the oldest stratum of the protolexicon, instead of an anachronistic jumble of items from several different temporal strata. We must no longer reconstruct on the basis of the IE cognates alone, throwing in the attested Hittite or other Anatolian cognates for good measure (as is the case in Pokorny's classic *IEW*, Gamkrelidze and Ivanov 1995, Melchert's *Anatolian Historical Phonology*, and in Rix's *Lexikon der idg. Verben* 1998, which purports to be an up-to-date revision of Pokorny's *IEW*). It must be noted, however, that some items may be missing simply because we have not yet found them. They may be hiding behind cuneiform sumerograms, like the Hittite word for _woman_, or behind Hieroglyphic-Luwian logograms, or they may not have surfaced at all. It is important to keep in mind the provisional nature of the "not found" label, but the great age (approximately 7000 years, i.e., going back to 5000 B.C., my rough PIH date) of what _has_ been found is certain. It is also important to keep in mind that the absence of a cognate in PA and PIE does not mean that the object denoted by it was unknown in the PIH community. It means only that the word cannot be said to have been formed during the period when the future Proto-Anatolians and Proto-Indo-Europeans formed a community that continuously shared the experiences of its intellectual, spacio-temporal, social, and economic life. Let us now look at what archaeology and history cannot—are powerless to!—give us: a glimpse of the intellectual and spiritual world, at the world of ideas that once distilled and defined the experience of these long-vanished folk, the Proto-Indo-Hittites.

In the area of _intellectual activity_, PIH had *_man-_ (to think) (*_man-ája-_ in Proto-Luwian: *_manaa-_ [to behold], *_man-ja-_ [to mind] > Luw. _maali-_, Lyc. _mali-ja-*_), which probably meant _to dwell_ (_on_) originally. Lyc. _Malija_ (Athena) may well be derived from Luwian *_mal-ja-_ (to think),[16] dissimilated from *_man-ja-_ (cf. Lyc. _alāma_ [name] < *_anama-_, cf. Hitt. _laaman-_ [name] with the same dissimilation) and thus equivalent to Homeric _Méntēs_ and _Méntōr_, the two avatars of Athena in the Odyssey (books I and II, passim), with the root exactly identical while the agent suffix is the well-known Anat. *_-ija-_. Apropos of Athena, the Lady of Wisdom, Attic _Athēna_ (Doric _Athāna_) looks to me like a gift from Hittite _hattannaš_ EN [=_išhaaš_], an epithet of Ea and Kumarbi, "the Lord of Wisdom," cf. Myc. dat.sg _a-ta-na-po-ti-ni-ja_ KN V52. The Hittite source of _Athāna_ is the genitive of _hattatar_ (wisdom) (see Lehrman 1998).[17] Other intellectual items that may be reconstructed for PIH include *_ǵnē-_ (know) (Anat. inferred from the Hittite punctual _ga-ne-eš-_ [recognize] from *_ǵnē-s-_): IE *_ǵnē-_/*_ǵnō-_, IH *_H_^{w}(_a_)_rd_^{h}_-_ (Anat. attested in Hitt. _hu-wa-ar-t+_ [to curse]) : IE *_werdh-_/*_wordh-_ (E. _word_ < *_werdh-_ , Lat. _verbum_, Lith. _vaȓdas_ [name]), IH *_war-_/*_war-ja-_ (speak) (Anat. *_war-_[_ja-_] > Hitt. _-war_ [quoth], Pal. _wer-ti_ [says], Hitt. _wer-je-_ [speak]: IE *_wer-je-_ > Gk. _eírei_ < *_wér-je-ti_, Russ. _vr-e-t_ [is lying] from a thematized zero-grade form of the root stem), IH *_l_(_a_)_is-_ (to track, trace, from _plow over_; furrow, track) (Hier. Luw. _arha li-sa_$_{8}$_-_ [plow over]) : IE *_l_(_e_)_is-_ (> P.Gmc. *_laizō_ [lore, learning] from *_lois-ā_ [tracking]; zero grade in P.Gmc. *_liznōn_ [to follow a course] > OE _leornian_ [to learn]; Lat. _líra_ [furrow] < *_leisā_). The PIH word for _name_ is *_na-mn̥_—the same in PA as attested in Hitt. _laaman_ with dissimilation of *_n_ to _l_, PL *_anə-man_ (with an anaptyctic vowel between _n_ and _m_) from the oblique stem *_n̥-mn-_ dissimilated to HL _atama_(_n_)_z_(_a_) and Lyc. _alāma_ : IE *_nō-mn̥_/*_n̥̄-m_(_e_)_n-_ > Ved. _nāma_, Lat. _nómen_, Gk. _óno-ma_, _énu-ma_, OCS. _imę_ < *_į̄mę_ < *_n̥̄-men_, etc.). PIH had the root noun *_djaw-_/*_dju-_/*_diw-_ (heavenly light) personified as the name of a deity of sunny or stormy skies (Anat. *_tjaw-_/*_tiw-_ : IE *_djew-_/*_diw-_). PIH had the verb *_spánd-_ (pour a libation) > Anat. *_spánt-_ attested in Hitt. _hhi_-conj. stem _išpant-_/_šipant-_, corresponding to IE *_spénd-_ (Gk. _spéndō_, etc.). The thematic adjective reconstructed in IE *_déjwo-_ (celestial), substantivized as _god_ (Lat. _deus_, Lith.

dievas, OE *tīw* [god of war] as in *tīwes dæg* [*mártis diés*]), is not attested in Anatolian. *Father Heavenly Light* of the IE **djēws* **pətēr* is also strictly IE, because the word **pətēr* (father) is unknown in Anatolian. The kenning for *Mother Earth* (the broad one) is also of post-IH age, since IE **pl̥təwī* fem. (Ved. *Pr̥thivī́*, OE folde) does not occur in Anatolian.

Among the items of landscape, we have the word for *earth* in IH as a neuter **dʰágʲʰm̥*/**dʰgʲʰm-í* or **dʰgʲʰám-i* loc. (Anat. **thákʲʰm̥* > Hitt. *teekan*, thematized **tʰákʲʰm-a-* > HL *takami-*, **tʰkʲʰám-a-* > CL *tijammi-*; IE **dʰgʲʰóm-* fem.). The PIH *sky* is **nabʰas-* (Anat. **nabʰas-* > CL *tappašša* [heaven], HL *tapasal/i-*, Hitt. *nepiš-*, umlauted from PA loc. **napʰasi* : IE **nebʰos* [(cloudy) sky]). Although the word for *star* in Anatolian is attested only in the Hittite *hapax hašterza*, with the final *-za* still not satisfactorily explained, it is tempting to postulate for it the PA age and reconstruct, on the basis of Anat. **h*(*a*)*star-* and IE *(*ə*)*ster-* (in Ved. *stŕ̥-bhis*, *tā́ras* [nom.pl], Gk. *astēr*, Lat. *stélla*, Go. *staírno*, etc.), IH **x*(*ə*)*star-*. Curiously, the *sea* cannot be reconstructed for PIH, testifying perhaps to the landlocked position of the original PIH-speaking area, but it is interesting that Hitt. *aruna-* (sea) points to Anat. **ar-un-a-* (the mobile one), an epithet derived from **ar-wr̥*/**ar-un-*/**ar-wan-*/**r̥-wan-* (that which is mobile, changeable), which is matched in IE by Greek *ouranós* (sky) < IE **or-wn̥-o-s* (the mobile one: no initial laryngeal reconstructible in the root!), the same epithet, isomorphous to Anat. **aruna-*. I cannot, however, be sure of this epithet's PIH age because it is attested only in Anatolian and Greek and may be due to some old contact. The word for *river*, IH **xabʰ-*, may be reconstructed from Anat. **hapa-* (Hitt. *hapaa* [to the river], CL *haapiš* (water) and IE **abʰ-* (> O.Ir. *aub*, Cymr. *afon* [river]). We have IH **wádr̥-*/**ud-an-* (water) in Anat. **wát-r̥* id. (> Hitt. *waatar*/*úiteni* loc. i-umlaut from **wadani*, whence *úitenaš* and the rest, the collective *úidaar* [waters] replacing **údaar* on the analogy with *úiteni*, etc.) and IE **wód-r̥*/**ud-én-*. No *mountains* seem to be in evidence, although the adjective *high*—Anat. **p⁽ʰ⁾r̥kʲ⁽ʰ⁾u-* (Hitt. *parku-*, CL *parrai-*) and IE **bʰr̥gʲʰu-* (high), the source of the word for *mountain* in Germanic and the arguable source of the word for *shore* in Slavic—is of the Indo-Hittite age. Hittite *hekur* (summit), sometimes compared with the IE root **akʲ-* ([sharp, tall] rock), cannot be matched up with any complete IE word and is likely to be a loan from Sumerian *É.KUR* (mountain temple) besides, as per Puhvel 1991. The most basic spatial (and metaphorically temporal) relations are shown by IH **and-* (interior), **anda*(*m*) (into) (Anat. **anta*[*m*] > Hitt. *anda*, CL *aanta*, Lyc. *ñte* : IE **éndo* > O.Lat. *endo*), **ap-* (back), dir. **apa*, loc. **api* (on), acc. **ap-m̥* (Anat. in Hitt. *aappan*, *aappa* (back[ward]) : IE **ap-*/**ep-*/**op-* in Gk. *apó* (from), *epí* (on), Myc. *opi*, Gk. *ópisthe* (back), etc.), acc. **km̥-ta-m* (down[ward]) (Anat. **kanttan* dissimilated in Hitt. *kattan*, dir. *katta* [down] : IE **kom-*, **km̥-tm̥* (downward) in Gk. *kata*; **par* (surface), loc. **pári*, acc. **pár-m̥*, dir. **pará* (> Anat. **par* [hence neu. **par-n-* (house?)] > Hitt. *pa-*/*pe-* (forth), HL *pa-*, loc. **pari* > Hitt. *peeran*, *peean* (in front), *paraa* (forth) : IE **per*, **peri*, **pro*, etc.). The word for *place* may be reconstructed as IH **pādam* (Anat. **pātam* > Hitt. *peedan* : IE **pēdom*).

We will speak next of words associated with calendar and time. Besides **djaw-/*diw-*, mentioned above in the meaning *day* with suffixes (e.g., Anat. **tiwatt-* in Hitt. *šiwatt-* [day], cf. Luw. and Pal. *tiwatt-* [sun]), IH had **nák^{w}t-* (evening) (Hitt. *nekuz* with i-umlaut < Anat. **nák^{w}ti* loc. [in the evening] : IE **nek^{w}t-/*nok^{w}t-* [night]) and **ksap-/*ksp-* (night) (Anat. **ksp-ant-* in Hitt. *išpant-* [night] : IE **ksep-* in In.Ir. **kṣap-* > Mod.Pers. *šæb* [night]); **wat-* (year) (Anat. **watt-* : IE **wet-*), **g^{jh}jam-/*g^{jh}ajm-/*g^{jh}im-* (snow, winter) (in Anatolian—but not in IH because no exact IE equivalent has been found—this root produces the word for *field*, **k^{jh}imra-* > Hitt. *kim*[*ma*]*ra-*, CL *immari-*, Lyc. *ipre-**; cf. Russian *ozímyja* [*poljá*], fields for sowing in autumn and harvesting in spring, lit. *about-winter*, i.e., both ends of winter), **wásx-* (spring) (Anat. in Hitt. *hamešha-*[*nt-*] < **ham-wash-* : IE **wés-r̥/-n-*), **san-* (autumn) (Anat. **sani* [in the autumn] > Hitt. *zeni id.*, nom. *zena-* [autumn] : IE *[*e*]*s-en-* [harvest time]). Anat. **jara-* ([a length of] time) can be reconstructed, thanks to Morpurgo Davies' (1987: 218, n. 31) etymology of CL *aara/i- c.* (time) and, based on the IE cognates (IE **jōr-ā*, a lengthened o-grade "*ā*-motion" feminine derivate of IE **jer-*, in Greek *hōrā* [hour, season], as well as IE **jēr-o-m*, a lengthened-grade derivate of the same root, whence Proto-Germanic **jēraN*, OE *gēar* [year]; also Proto-Slavic **jaro* [spring/summer season]), the root **jar-* (time, duration) may well be projected into PIH (Hitt. *mehur*, without Anatolian cognates and full-word connections in IE, remains obscure.)

For IH plants we have **daru* neu. (tree) (Anat. **taru* : IE **dóru*) and **jáwa-* (barley) (Anat. **jawa-* > Hitt. *ewan* neu. : IE **jéwo-* > Ved. *yáva-*, Hom. *zeiá* [spelt], Lith. *javaĩ* [crops]). *Wheat*, *rye*, and *flax* cannot be reconstructed for PIH, nor can any specific tree names such as *birch* or *beech* be posited for the PIH period. It is possible that Hitt. *eja-* (sacred evergreen tree) (< *eternal?*) goes back to PIH **aj-* (originally *ever?*) (Anat. **aj-* > Hitt. *ej-* plus the thematic suffix = *eja-* : IE **ej-wo-* [yew]; in O.Ir. *eo* id.; OE *īw*, *ēow*; Russ. *íva* [willow], etc.).

The following IH animals can be reconstructed: **Haran-* (eagle) (Anat. **haran-* : IE **orn-*), **Hawi-* (sheep) (Anat. **hawi-s* > CL *haaúiiš*, Lyc. *xawã* acc. : IE **owis* > Gk. *óFis*, Lat. *ovis*, etc.) with **H^{w}l̥̄na-* (wool) (Anat. **H^{w}l̥̄na-* [wool] > Hitt. *hulana-*, CL *SÍG-laniš* id. : IE **wl̥̄n-ā* > Ved. *ū́rṇā*, Lat. *lána*, Lith. *vìlnos*, etc.), with the verb root **pax-* (to guard, pasture) (Anat. punctual **pahh-s-* : IE **pā-* and **pā-s-*), IH **wl̥k^{w}á-* (as an adjective *dreadful*, substantivized in Luw. *walwa/i- c.* [lion] and attested in Hitt. adj. *walkuwa-* [dreadful]), IH **xŕ̥tk^{j}a-* (bear?) (Anat. **hr̥tk^{j}a-* > Hitt. *hartagga-* [bear?] : IE *ŕ̥tk^{j}o-* [bear] > Ved. *ŕ̥kṣa-*, Gk. *árktos*, etc.). *Bees* can be safely inferred from IH **málit-* (honey) (Anat. **málit-* > Hitt. *melit-*, CL *mallit-* : IE **mélit-*) and IH **mád^{h}u-* (mead) (Anat. **mat^{h}u-* < Hitt. *maddu* [mead] : HL *matus*[*á*] id. : IE **médhu* > Ved. *mádhu*, Hom. *méthu*, etc.). We can reconstruct IH **g^{w}aw-* (bovine) (Anat. **waw-* > HL *wawi-* [cow], Lyc. *waw-* id. : IE * *g^{w}ow-*)—but a cognate of IE **melg^{j}-* (to milk) has not yet surfaced in Anatolian and so cannot be placed on the PIH level. On the other hand, IH **k^{j}wan-* (dog) (= Anat. > HL *zuwani-* id. : IE **k^{j}won-*) and IH **ák^{j}wa-* (horse)

(> Anat. *_akk^{j}wa-_ > CL _azzuwa-_ : HL _ázúwa/i-_ : Lyc. _esbe_ [horse] : IE *_ék^{j}wo-_ id.) are both present.

Neither IE *_g^{jh}wer-_ (wild animal) nor IE *_pék^{j}u-_ (cattle) has a cognate in Anatolian, thus no comparable PIH words may be reconstructed. Instead, Anatolian uses *_h^{w}itár-_ (Hitt. _huitar_ [animals] coll.), apparently derived from the verb *_h^{w}aj-_ /*_h^{w}i-_ (move, run)—as also the Hittite verb _huiš-_ (live) seems to be (Anat. *_h^{w}i-s-_ [to make a move], by implication _to be alive_, derived from Anat. *_h^{w}i-_ [to move] by means of the punctual suffix *_-s-_ surviving into PIE and innovatingly redeployed there as the marker of *_s-_ aorist]. Quite a few well-known members of IE lexical fauna, including domestic animals and pests, have _not_ so far been found in Anatolian: ducks (IE *_ánət-_), geese (IE *_g^{jh}ans-_) or birds in general (IE *_awi-_), hedgehogs (*_eg^{jh}i-_), pigs (*_sū-_) or piglets (*_pórk^{j}o-_), lambs (*_wren-_), oxen (*_uksén-_) or bulls (*[_w_]_ŕ̥sen-_), deer (*_elen-_), hares (*_k^{j}osó-_), ants (*_morwi-_), wasps (*_wobhsā_), mice (*_mūs-_), lice (*_k^{j}nid-_), worms (*_k^{w}mr̥i-_), snakes (IE *_og^{wh}i-_ and *_ang^{wh}i-_) have not been found. Salmon (*_lok^{j}so-_), fish (*_d^{h}g^{jh}ū-_), and mussels (*_k^{j}onk^{h}o-_) are also missing.

As for the human beings themselves, IE *_mánu-_ (Ved. _mánu-_, G. *_manw-_ whence E. _man_), *_wīro-_ (Ved. _vīra-_, Lith._výras)_, *_ənér-_ (Gk. _anēr/andr-_), and *_d^{h}g^{jh}em-on-_ (whence Lat. _homo_, OE _guma_, Lith. _žmónės_ [people]) are not found in Anatolian and thus cannot be reconstructed for PIH Hittite _antuwahha-_, _antuhša-_ (human being) has been attractively etymologized (Oettinger 1978: 373, under Hitt. _tuhhai-_ [choke, pant]) as derived from *_en_ (in) + *_d^{h}wahh-_+ (or +*_d^{h}uxs-_, zero grade of neu. *_d^{h}aux-as-_ [breathing, spirit]) + *_-a-_ (adjectival suffix) = _he/she in whom there is breathing/spirit_), but this complex of morphemes cannot be posited for the PIH level in the meaning _human being_. Instead we get IH *_pāsna-_ (man) (Anat. *_pēsna-_ > Hitt. _pišna-_), probably a lengthened-grade thematic derivative from *_pasn-_ (an oblique stem of m. *_pasār_ or n. *_pasr̥_ [penis]), matching IE *_pēsn-i-_ in Lat. _pēnis_. IH *_g^{w}ana-_ (woman) may be reconstructed on the basis of Anat. *_wana-_ (CL _waana-_) : IE *_g^{w}(e)nā_ (Gk. _gunē_, OI. _ben/mná_, OCS. _žena_, etc.). It is likely that Hittite _wen-_ (_futuere_) is formed from the same root as the word for _woman_: just as the word for _man_, *_pāsna-_, is derived from _penis_, so the word for _woman_, *_g^{w}ana-_, may well be derived from the root *_g^{w}an-_ (IE *_g^{w}en-_) _vulva_. Cf. K. T. Schmidt's etymology of Gmc. *_wība-N_ neu.(wife) < IE *_g^{h}wejb^{h}o-m_ (shame) > Toch. A _kip-_, B _kwipe-_ (genitals, shame) (Schmidt and Strunk 1989: 251ff).[18] Speaking of love-making, IE *_jeb^{h}-_ (_futuere_) (Ved. _yábh-a-ti_, Gk. _oíph-e-i_, Russ. _eb-ë-t_) is not in Anatolian and hence cannot be reconstructed for PIH. Among related activities is *_swapr̥-/*supr̥-_ (sleep) (Anat. in Hitt. denominative _šupparija-_ [to sleep] : IE *_swép-n-/*sup-n-_ in Ved. _sváp-na-_ [sleep], Gk. _húpnos_, OCS. _sŭn-ŭ_ [sleep], _sŭp-a-ti_ [to sleep]). Multipurpose is *_ap-_ (to grasp, grab) (Anat. *_app-_ in Hitt. _eipzi_ [seizes] [with i-umlaut], _appanzi_ [they seize] : IE *_ap-_ in Ved. _āp-nó-ti_, Lat. _apió_).

Not found in Anatolian are the root *_g^{w}ī-_ (to live) and its derivative *_g^{w}ī-w-o-_ (alive). Anatolian has *_h^{w}i-s-_ (to live) (< *_γ^{w}i-s-_) instead, derived from *_γ^{w}i-_ (the zero-grade form of *_γ^{w}aj-_) _to move quickly_, mentioned previously, with its derivatives

*_h^wi-s-u-_ (alive) and the _n_-infixing factitive *_$h^w$is-nau-_ (Hitt. _huišnu-_) (to revive). Whether IE *_(ə)wés-_ (to stay overnight) is a cognate or not is at best uncertain (cf. Lehrman 1998, where Hitt. _huiš-_ and IE *_[ə]wés-_ are mistakenly, in line with the _communis opinio_, regarded as cognates). IE *_ánə-/*anḗ-_ (breathe) and derivatives (Gk. _ánemos_ [wind], Lat. _animus_ [spirit], etc.) are not found in Anatolian and thus not in PIH. IE *_mer-_ (to die) is matched by Anat. *_mar-_ (disappear); thus IH *_mar-_ (disappear) may be reconstructed. Hittite has _ak(k)-/ek-_ (_hhi_-conj.) (die), not attested in Luwian, which has not been provided with a satisfactory etymology as yet, despite the many attempts (Rix's LIV has **_$h_1$ek-_, with the note "Nur anatol."; needless to say, the first laryngeal is a phantom, as is the "perfect" form **_h_1e-$h_1$ók/h_1k-_, meant as the prototype of the Hittite _aaki_ (dies)/_aakkiš_ (died) as per Oettinger 1978: 401–3). An obvious explanation, not as far as I know attempted by anyone as yet, would be to regard IE *_nék^j-/*n̥k^j-_ (kill; perish) (Ved. _naśanti_ [they will perish], Lat. _noceó_ [_harm_, originally _kill_]) as a post-Anatolian IE remodeling of the *_-n-_infixing transitive IE *_é-n-k^j-/*n̥-kj-_ based on the root *_ak^j-_ (to die) that would then be of IH age.

For _giving birth_ Anatolian has *_has-_ in Hitt. _hašš-_ (_hhi_-conj.) apparently connected with _haššu-_ (king) in the same way as Germanic *_kun-ing-az_ (> E. _king_, etc.) is connected with PG. *_kun-i-_, *_kun-ja-_ (clan, kin) built from the zero grade of the IE root *_ǵénə-/*ǵn̥̄-_ (give birth), a root not with certainty found in Anatolian; IE *_ansu-_ found in Avestan _ahu-ra-_ (lord) and Germanic *_ansu-_ > ON. _áss_ (pl. _æsir_) (a god) may be a cognate of Anat. *_has-_, in which case an IH *_xansu-_ (king) could be reconstructed, with some reservations.

Important body parts other than the genitalia, that can be reconstructed for IH, are *_pada-_ (foot) (Anat. *_pata-_ : IE *_pod-/*ped-_), IH *_g^{jh}asr̥_ (hand) (> Anat. *_k^{jh}asr̥_, loc. _k^{jh}asri_ > Hitt. _keššar_, loc. _kišri_, CL _iš(ša)ri-_, HL _is(a)t(a)ra/i-_, Lyc. _izre_ : IE *_g^{jh}ésr̥_ > Gk. _kheír_, etc.), *_ǵánu-_ (knee) (PA *_k^jánu-_ > Hitt., Pal. [?] _genu-_ : IE *_ǵénu-_ (j-umlaut)/*_ǵónu-_ (u-umlaut)/*_ǵnéw-_ > Gk. _gónu_, Lat. _genú_, Goth. _kniu_). PIH *_xant-_ may be reconstructed in the meaning _forehead_ (Anat. *_hant-_ : IE *_ant-_). _Heart_ is IH *_k^jārd/*kjr̥d-_ (Anat. *_k^jēr_ in Hitt. _kir/kart-_, Luw. _zarz[a]_ : IE *_k^jēr[d]/*k^jr̥d-_). _Bones_ is *_xast-aj_ (Anat. *_Hast-aj-_ > Hitt. _haštai_ [bones], C.Luw. _haašša_ id. : PIE *_ost-_ > Gk. _ostéon_, Lat. _os_, Arm. _oskr_), and _blood_ is *_ásHr̥/*sH(a)n-ás_, fact. *_sHr̥-naw-_ (to bloody) (Anat. in Hitt. _eešhar_, gen. _išhanaaš_ and _eešnaš_ (_Telipinuš_), instr. _išhanda_, vb. fact. _išharnu(w)-_, C.Luw. _aašhar(ša)_ n. _blood_, fact. _aašharnu-_ :: PIE *_ésr̥/*es-n-os_ > Ved. _asr̥-k/_gen. sg. _asnás_ (AV), Toch. B _yasar_, A _ysār_.

The only kinship terms that can be reconstructed for IH are *_Hawxa-s_ (grandfather) (Anat. *_hauhhas_ > Hitt. _huhhaš_, CL _huuhaš_, Lyc. _xuga-_ : IE *_awo-s_ > Lat. _avus_, etc.) and *_dhug(ə)tar-_ (daughter) (Anat. *_t^hukattar-_ > HL _tuwatara/i-_, Lyc. _kbatr-_ : IE *_d^hug(ə)tér-_ > Gk. _thugátēr_, Ved. _duhitár-_, Lith. _duktẽ_, etc.)—nothing else. IE *_mā́tēr_ (mother), *_pətḗr_ (father) (not attested in Balto-Slavic!) with *_pətr̥wijo-_ (father's brother), *_bhrā́tēr_ (brother), *_swésōr_ (sister), *_sūnús_ (son) (not attested in Italic; Gk. _huiós_ pointing to *_sūyós_), *_népot-_ (_grandson_ or _sister's son_?), *_neptī́_ (_grand-_

daughter or *sister's daughter*), *snusós (son's wife), **swékʲuro-s* (husband's father), **swekʲrū́s* (husband's mother), **swēkʲuró-s* (wife's brother), **daiwḗr* (husband's brother), **gʲ(e)ləwó-* (or the like) (husband's sister) (Gk. *gálōs*, Lat. *glōs*, OCS *zŭlŭva*), **y(e)nətḗr* (husband's brother's wife) (Ved. *yấtar-*, Lith. *jéntė*, OCS *jatry jatrŭve*), **gʲm̥̄--*, **gʲemə--* (daughter's husband) (Skt. *jāmātár-*, Gk. *gambrós*, Lat. *gener*, Russ. *zjat'*, Lith. *žéntas*), **swéliyo-* (wife's sister's husband)—the entire IE brood is unknown in Anatolian and therefore in PIH, perhaps with far-reaching consequences for what can be said about PIH family structure and family values. The root of a word for something like *tribe* or *troop*—Anat. **taut-ija-* (> Hitt. *tuzzi(ja)š* [army]) and IE **teutā* or **toutā*—can be reconstructed for PIH as **taut-*, with the suffix uncertain.

Among IH general economic terms are **Hap-* (wealth) (Hitt. *happin-ant-* [rich], *happin- ahh-* [make rich], suggesting a thematic *happina-** [ṛich], *happinešš-* [become rich] : PIE **op-* > Lat. *op-s* [riches]—Gk. *áphenos* [riches], *aphneiós* [*rich*] less likely), **tā-* (steal), which presupposes the notion of property, **was-* (sell) (**wás-a* [to him is sold], in Hitt. *-hhi* conj. *waaši* < **was-a-i* [buys]; in IE found only in nominal derivatives such as Lat. *véno-* [sale] < **wes-no-*, whence Lat. *véndere* [to sell]).

Among technological items are IH **Hassa-* (hearth) (Anat. **Hassa-* [hearth] > Hitt. *hašša-* [hearth], C. Luw. *haššanitti-*, Lyc. *xahati* abl./instr. [altar] : PIE **ās-*? > Osk. *aasa-*, Lat. *āra* [altar]), **paxʷ-r̥/*p(a)xʷ-an-as* (fire) (Anat. **pahhʷ-r̥/*p[a]hhʷ-an-as* > Hitt. *pahhur/pahhuwar*, loc. sg. *pahhueni*, analog. gen. sg. *pahhuenaš*, C.Luw. *paahuur* : IE: Gk. *pûr*, etc.), **Har-s-* (to plow) (Hitt. *har-š̥-* [to plow]), **l(a)is-* (track, furrow), **jugá-* (yoke), **Hissa-* (wagon pole) (Hitt. ^{GIS}*hišša-* :: Ved. *īṣā*), **mlā-/*mál(ə)-* (grind), **nagʷ--* (naked), with the implication of the existence of clothes, confirmed by **was-* (wear) (factitive **was-ája-* [to dress, clothe]), the punctual **takʲ-s-* (to fashion, fit together [wood]), **Hʷl̥̄nā* (wool) (Hitt. *hu-la-na-aš*), **pāx-s-* (to guard, pasture) (Hitt. *pa-ah-ha-aš-*).

Not (yet) found in Anatolian are IE **áyos-* (*metal*, source of E. *ore*), **əlḗ-/*álə-* (to grind), **sé-* (to sow) (Hitt. *šehur* [urine] is certainly not from this root), **sal-* (*salt*, notably also not found in Indo-Iranian), **dʰeigʲʰ-* (to knead, mould clay or dough), implying pottery and baking, **dʰwor-* (door) and, derived from it, **dʰworo-* (courtyard), **yā-* (ride), implying domesticated animals to pull wagons or sleds or to be ridden directly, **rē-/*erə-* (to row), **au-* (put on gloves or shoes), **gʲómbho-* (peg; tooth of a rake or comb), **gʷr̥̄won-* (millstone) (if lit. *having roughness*, then at least its components are attested in Anat.), **yes-* (to boil, ferment), **pekʷ-* (to cook [by roasting, broiling, or baking]), **yūs-* (soup), **yōs-* (to gird), **ksuróm* (razor), **kʷékʷlo-* (wheel), (root **kʷél- turn* and its derivative *ring* are found in, e.g., Hitt. *kula-* [(ear)ring] < **kʷl-a-* or **kʷal-a-*), **rótho-* (wheel) (root **reth-* [run'], **kʷeru-* (kettle) (also **kʷor-y-ā*, cf. O.Ir. *coire*, *core* [cauldron]), **nāu-* (boat), **pleu-* (to swim, float, sail), **neigʷ-* (to wash), **n̥si-* (sword), **kʲéru-* (*spear* or *dagger*), **pelekʲu-* (ax), **peikʲ-* (to paint, embroider), **peis-* (to crush) (e.g., in a mortar), **pl̥̄-* (fort, fortress, town),

**pontā-* : **pn̥t(ə)-* (path), **pr̥tú-* (ford), **tl̥̄tó-* (ford) (all three of these came to be used for *bridge* in one or more branches—*pont-* in Latin, *tílta-* in Baltic, *pr̥tú-* in Iranian), **pōtlom* (drinking vessel) (IH **pa-s-* [to swallow] is found in Hitt. *paš-* [with no laryngeal whatsoever]), **syū-* (sew), **tékʲ-on-* (carpenter) (the root **takʲ-* [to fashion], augmented with the punctual suffix **-s-*, is found in IH), **webʰ-* (to weave), **gʷiyā* (thread, bowstring).

The only numerals we can reconstruct with confidence for PIH are those found in both Anatolian and IE: **dwa-*(2), **tri-* (3), **saptḿ̥* (7), and **náwn̥* (9). Not (so far) found in Anatolian are the cognates of PIE **sém-* (one) (the regular cardinal number in Greek, Armenian, and Tocharian); fem. **smī́*, **som-o-* (one and the same), **sm̥-* (some), **sēmi-* (one half), **ói-no-* (one) (the regular cardinal in Celtic, Latin, Germanic, Balto-Slavic, Albanian; Gk. *oínē* [the one on a die]), **ói-wo-* (one) (regular in Iranian; Gk. *oîos* [alone]), **ói-ko-* (*one*, regular in Indic), **kʷetwóres*, fem. **kʷétesr-* (4) (here Anatolian had a different word, **maw-* > Hitt. *mew-*, Luw. *maw-*), **pénkʷe* (5), **s(w)ékʲs* (6), **okʲtṓ* (8) (possibly a dual of a word meaning *the four fingers other than the thumb*), **dékʲm̥t* (10), **wíkjm̥tī* (20), **kʲm̥tóm* (100). There must have been words in PIH for 11–19 and for 21–99, but it is not easy to say exactly what their shapes were in PIE, let alone PIH. The word for *1,000* cannot be securely reconstructed even for PIE.

6

What I have offered here for public consideration and criticism is a mere sketch of the reconstruction of PIH.[19] Although some of the particulars of the reconstruction offered here remain provisional and will be revised as new factual and conceptual discoveries are made, the Indo-Hittite theory as a whole—established as it is on the firm foundation of the Neogrammarian comparative method—and its very substantial consequences for the shape of the protolanguage we can reconstruct are, to my mind, beyond any reasonable doubt. Just when we thought there was nothing left to do in Proto-Indo-European but to elaborate, in lengthy *Kleinarbeiten*, some rather boring little details, it turns out that we are standing on the threshold of an exciting new field of study in which the most basic work has yet to be done.

Notes

I thank Robert Drews and H. Craig Melchert for their helpful comments on an earlier version of this paper.

1. Private communication of January 28, 2000, Berkeley, Calif.

2. See, e.g., Melchert 1998. Judging by Professor Melchert's response to this paper and the subsequent discussion on March 19, 2000, it is apparent that he, too, has now accepted the name as well.

3. For recent critiques of the laryngeal theory see, e.g., Lehrman 1997, 1998: 257–64, and Lehrman forthcoming (a).

4. The nominalist "arbitrariness of the linguistic sign" is contradicted by the evidence of historical language study showing that every sign is perfectly motivated at every stage, and that its initial arbitrariness is something to be proved rather than dogmatically asserted. "Synchrony" is a mere snapshot of the continuum of change, and making it an equal (and de facto dominant) partner with "diachrony", or the unceasing continuum of change through activity, is at best naive.

5. The overprizing of consensus and of the number of adherents of an idea, as if these sociopolitical variables were methodological, logical, or epistemological criteria in judging the validity of a scientific theory, is remarkable also in the work of the outstanding language historian Craig Melchert, and reflects well the conformist atmosphere of the U.S. East Coast "establishment" and its offshoots. Cf., e.g., the following statements: "Until we come to a consensus regarding the position of Anatolian—and the related question of to what extent we must modify our reconstruction of Proto-Indo-European itself—even those whose focus is on the history of other subbranches risk basing their analyses on a proto-system that may suddenly be invalidated" (Melchert 1998: 24). "[The Indo-Hittite theory], never popular in Europe, now has relatively few proponents . . ." (25). "I do not mean . . . to say that the proponents of the [Indo-Hittite theory] did not continue to exist, but they were distinctly in the minority" (26).

6. Although both the old Indo-Iranian and Homeric texts were committed to writing several centuries after they had been composed, and the available witnesses are younger still, there is no reason to doubt the faithfulness and efficacy of the oral tradition that preserved them.

7. See, e.g., Ivanov 1992: p. 11f., where the "exceptional closeness" of the Tocharian consonant system to the Ob'-Ugric languages and some "lexical coincidences" with Common Ugric are described in some detail, with a reference to a 1927 publication by Schulze (reprinted in his *Kleine Schriften*, Göttingen: Vandenhoek and Ruprecht 1934, 257) where he acknowledges the "noticeable trace[s] of foreign influence" in the languages of the far-flung Tocharians.

8. There is also a tendency among Baltic and Slavic scholars to represent Baltic and Slavic as particularly archaic in certain regards, especially in their accentology. This alleged archaicity is also illusory, for similar reasons. I agree generally, if not in detail, with Jerzy Kuryłowicz (see Kuryłowicz 1968), on the innovativeness of the Balto-Slavic accentual system. As I show in my forthcoming (b), the Balto-Slavic accentual system is innovative and has nothing to contribute to the reconstruction of Proto-Indo-European, to say nothing of Proto-Indo-Hittite. It is the other way around: certain phonological phenomena of PIE may explain the sources of the Balto-Slavic accentual innovations, but this is significant only for Balto-Slavic.

9. See a critique of these views in Lehrman 1996.

10. As far as I know, Schürr and Melchert are working on a recently found bilingual that may improve our knowledge of Carian, thanks to the knowledge we have based on Hittite and the two Luwian dialects.

11. See Lehrman 1998, p. 265ff.

12. See Dunkel 1990 on how these developed into a pronominal paradigm out of sentence particles. In Dunkel's Meidian flux of Early, Middle, and Late IE, the paradigm comes into being in Late IE while in Middle IE **so* and **to* are still phrase-initial, but this is just a matter of terminology.

13. I cannot understand why basic vocabulary—or any vocabulary, for that matter—should have a constant rate of replacement. Until I find clearness on that fundamental tenet

of glottochronology, I cannot use glottochronology in good faith.

14. It is quite likely that the future Indo-Aryans, some of whom were those expert horsemen of Mitanni, soon to be en route to India, stayed among the Luwians long enough to adopt their three-vowel system.

15. See Melchert's brilliant 1985 paper (published 1987).

16. See Brown 1998: 56, with ref.

17. If Luwian _palahšaš_ meant _shield_ (some say it meant _cloak_), it would be attractive to etymologize Athāna Pallas as _hattannaš palahšaš_ (the Shield [or Cloak] of Wisdom).

18. E _cunt_ may be derived from the same root, IE *_g^{w}en-_, in the zero grade with an obscure suffix: *_k(w)un-tō_ < *_g^{w}n̥-dā_. Hitt. _wen-_ (_futuere_) may well come from PA *_g^{w}an-_ rather than from *_wan-_ (desire) (wrong **_wenH-_ [_liebgewinnen_] Rix 1998: 623: no trace of any laryngeal anywhere at all!) or *_wan-_ (win).

19. It is important to keep in mind that only the presence of a separate reconstructed word is significant in that it demonstrates the presence, in the reconstructed protolanguage, of the item denoted by the word. The absence of the word cannot mean that the item denoted by it was missing: either (1) the item may have been denoted by a different word or (2) the cognate word did exist and simply has not surfaced yet. To illustrate (1), the word for _wheel_ in Hittite was _hurki-_ (PIH root *_H^{w}r̥g-_ [turn]) while in PIE it was *_k^{w}ek^{w}lo-_ whence also E _wheel_ (PIH root *_k^{w}al-_ [(make]) round], well attested in various Anatolian derivates; see, e.g. Lehrman 1998: 148 with ref.). The use of two different words suggests perhaps a difference in the technology of wheel making (cf. Bill Darden's paper in this volume). It looks like PIE innovated by introducing a reduplicated noun for _wheel_ not (so far) found in Anatolian; this lexical innovation would be yet another example of PIE innovations as compared with Anatolian.

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SOUTHERN ANATOLIAN AND NORTHERN ANATOLIAN

AS SEPARATE INDO-EUROPEAN DIALECTS AND ANATOLIAN AS A LATE LINGUISTIC ZONE

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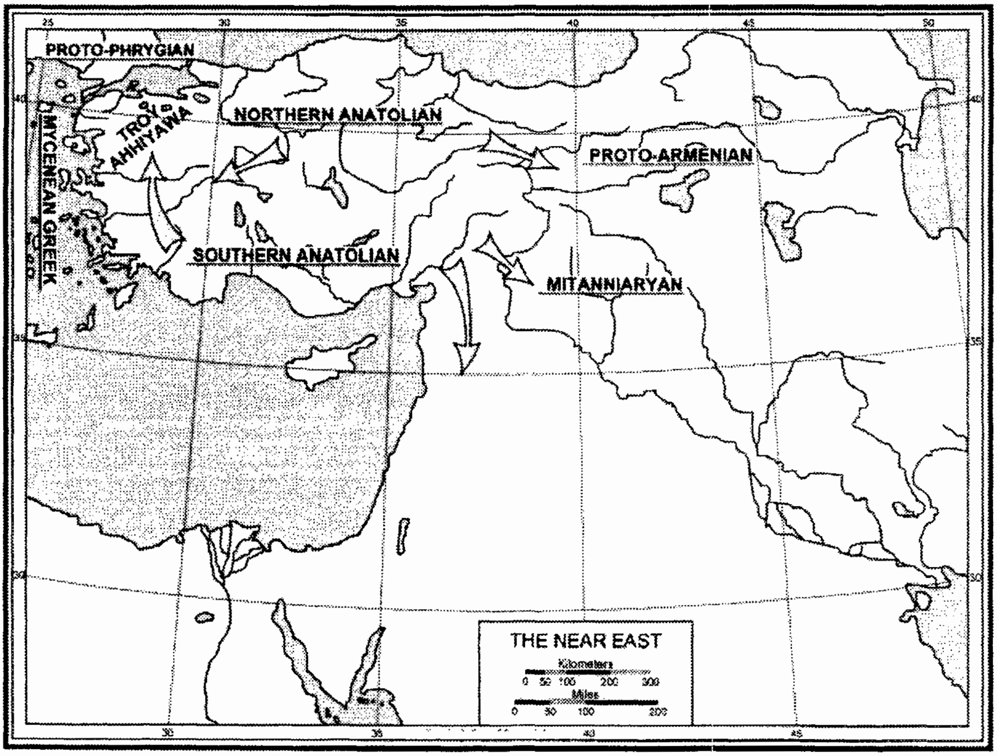
The topic of my talk belongs to a sphere that has been popular in Indo-European comparative studies of the twentieth century. As soon as a field of research on a branch of the Indo-European linguistic family becomes ripe enough, a question arises on the spatial and temporal links between the languages that are included in it. Scholars discuss not only the possibility of linking together two branches (as in the classical cases of Italo-Celtic and Balto-Slavic) but also the arguments for and against uniting their parts (e.g., Latin and Osco-Umbrian or Eastern and Western Baltic—Lithuanian and Latvian, on the one hand, Old Prussian and Curonian, on the other). Inside a group that seems homogeneous, a dialect or a whole group of dialects may be discovered that contradicts this impression, as Nuristani (Kati, Ashkun, Waigali, Prasun) among Indo-Iranian languages or the language of Old Novgorod birch-bark documents that differs from the rest of Slavic.

With the development of linguistic geography and the studies of common features of modern languages of one linguistic zone or "league" (*Sprachbund* of the type found, for instance, in the Balkans or in Meso-America), a strict dichotomous division of branches inherited from Schleicher's times slowly gives way to less rigid schemes going back to Johannes Schmidt's wave theory. Particularly important are the possibilities of finding traces of interconnections between different linguistic families, the members of which had been united for centuries inside social communities like empires (as in the case of Indo-European Anatolian languages and their non-Indo-European neighbors). At the same time inside a group united by such later links, one may distinguish traces of much older dialectal differences. In a bundle of dialects supposed to constitute a single branch of Indo-European (IE), differences between separate languages may be found that also occur as isoglosses dividing other branches of the family. Thus a picture of such a branch from the point of view of comparative IE dialectology may produce results different from an attempt to build a proto-system for this branch in a *Junggrammatiker*'s manner.

Following are features that separate Hittite (Northern or Eastern Anatolian, NA) from Luwian (Southern or Western[1] Anatolian, SA, see accompanying map) in a way comparable to differences between other autonomous branches of the IE family. I have enumerated fifty-three features in all and grouped them under several broad and subsidiary headings.

Northern Anatolian, Southern Anatolian, and Eastern Indo-European in the Second Millenium B.C.



Phonological Isoglosses

1. In *phonology*, particularly important seems the fact that while Hittite and the other Northern Anatolian languages belong to the centum dialects in which IE palatals merged with velars, in Southern Anatolian voiceless IE palatals developed into affricates/fricatives (satem[2] type): Old Hittite (OHit.) *ki-ir* (*kēr*), *kardi-* (heart), Gk. κῆρ, καρδία (heart), Gothic (Goth.) *hairto* (heart) < IE **k'ēr*, *k'r̥d-*: Cuneiform

Luwian (CLuw.) *zart-, zarza,* Hieroglyphic Luwian (HLuw.) *zarza* (heart), Old Prussian (OP) *ſßēr*, Old Church Slavonic (OCS) *sьrdь-ce* (heart);[3] Hit. *ki-* (this): Luw. *za-/zi-* (this), Lith. *šì-s*, OCS *sь-*; Hit. *ki-ta* (*ri*) (to lie, be put), Palaic (Pal.) *ki-tar*, Gk. κεῖ -ται: Luw. *ziy-ari* (to lie), Lyc. *si-*, Skt. *śe-te*, Avestan (Av.) *saē-te*. Since in these forms in Luwian and Lycian the development into affricates/spirants is attested in a position before frontal vowels, that could have been interpreted also as a later event giving a result similar to palatalization in the other satem dialects. But this alternative hypothesis is ruled out in those cases where the same change occurs in the other positions: Hit. *karawar* (horn), loc. *garaun-i[,* Goth. *haurn* (horn), CLuw. *zarwani(ya)*, HLuw. *sù-ra/i-na/i* (horn),[4] Lith. *stìrna* (roe, doe), OCS *srьna*; Hit. *kuwan* (dog[?]), Lyd. Καν (*dog*, in the name of Dog-Slayer, Κανδαύλης), Gk. κύων: HLuw. *sù-wa/i-ní(-i)* (dog),[5] OP *sun-is*, Skt. *śuva-*; IE (NA ?) proper name *Akuwa* mentioned in several Old Assyrian tablets from trade centers and probably originally connected to the Indo-European name of horse,[6] Latin (Lat.) *equus* (horse): CLuw. *azz/ššuwa-* (horse), HLuw. *á-sù-wa/i*, Lyc. *esbe-*, OLith. *ašvà* (mare), Skt. *áśva-* (horse);[7] Hit. *aku-* (stone), Lat. *acu-s* (needle), Goth. *ahs* (ear of grain, ear), Gk. ἄκμων (anvil < stone): HLuw. *á-su-* (stone), Skt. *aś-man-* (stone).

2. In the initial position in some words IE *s-$_{mobile}$[8] > Hit. *š-*, but in Luwian it develops into *t-*: Luw. *tawa/ī* (eye) (Lyc. *tawa*, collective pl.)[9] < *takwa : Hit. *šakuwa* (eye) (OIrish [OIr.] *ro-sc*), Hit. *šakuwai-*: Goth. *saihvan* (to see) (Eng. *see*), Hit. (in mythological epics) *šakuwai-šk-* (iterative): Albanian (Alb.) *shoh*.[10] In the other dialects outside of Hittite this initial consonant has been preserved only in a verb while in the corresponding noun *s$_{mobile}$ - > *0*: IE dial. *ok^{w}- (eye) < *s$_{mobile}$$ok^{w}$-o. Another IE word with the same initial is present in Hit. *šankui-* (nail) (*s$_{mobile}$- > *0* in IE dial. *s$_{mobile}$[o]$ng^{w}h$- (nail): Lat. *unguis*, OIr. *ingen*). A trace of a "prothetic" vowel (*$_{e/o}$) after the loss of an initial *s$_{mobile}$ has been preserved in Gk. ὄνυξ, όνυχς and probably in Armenian (Arm.) *eługn* (nail, claw). The vowel has been lost in Germanic (Goth. *ga-nagljands* [nailing on], Old Norse [ON] *nagl*, Eng. *nail*), Balto-Slavic (B-S), and Tocharian (Toch.); in Italo-Celtic it is not easy to distinguish a possible trace of a reduced vowel from a syllabic form of an initial sonant. Luw. *tammuga* (nail [clippings]) < *s$_{mobile}$$omgh^{w}$- has the same dialectal *m as in Toch. A *maku*, Toch. B *mekwa*. In OP *nagu̱tis*, Lith. *nãgu̱tė*,[11] OCS *nogъṯь* (nail) there is a *-t-* element either produced by a distant metathesis (cf. a partly similar relation of Arm. *eługn* [nail, claw] to ON *nagl* and Eng. *nail*) or belonging to a final suffix of another origin (absent in Lith. *nãgas* [nail], Sl. *nog-a [foot] from a collective form of the stem grammatically equivalent to Luw. *tammuga*, Toch. B *mekwa*). A similar initial can be found in Hit. *šehur* (urine) (*s$_{mobile}$ > *O* in Lat. *ūr-ina*)[12] that corresponds to Luw. *dūr* (urine).

A specific development making this phoneme in Luwian different from the reflex of *s > *š* may be connected to the satem-type change of voiceless palatal stops. As the latter developed into affricates and spirants, the former spirants had to change. A "chain reaction" in the phonemic system may be compared to a similar one that led to the contextually motivated development **s* > *š* in some satem dialects (Indo-Iranian with a specific development in Nuristani, Balto-Slavic, Albanian).

3. Southern Anatolian coincides with Eastern Baltic in denasalization of the initial phoneme **n-* > *d-* in the name of a sky: IE **nebhes-* > Hit. *nēpiš-* (sky) (Skt. *nabhas-*, Sl. **nebo*, oblique stem *nebes-*): CLuw. *tappaš-*, HLuw. *tipas-* (sky)[13] : Lith. *debesìs*, Latvian (Latv.) *debess* (sky).

4. Completely different in the two languages is the phonemic shape of an Indo-European word for *mouth*: pre-IE ***oGis* > **oHis-* > Hit. *aiš*, oblique *išš-* (mouth) (Hittite is unique among IE language in having preserved the structure of the word and an *-i-*, although it lost an intervocalic consonant).[14] Luw. *a-a-aš-ša* (the spelling may point either to a pronunciation of a laryngeal not expressed as such in the cuneiform writing or to a length or a hiatus caused by its disappearence) is structurally similar to Lat. *ōs*, Skt. *ās* (mouth < **oHs*).

Prothetic Vowels in the Initial Syllable: Luwian and Greek-Armenian

5. In Luwian the development of laryngeals in the initial position and in the middle of a word is different from that in Hittite. For the initial position representative is IE **Hno-m(e)n* > Hit. *laman* (name) with a zero reflex of the initial laryngeal as in most dialects besides the Greek-Armenian part of the Eastern IE area. HLuw. *a-ta*$_{4/5}$*-ma -za* (name) has a prothetic vowel of a type similar to a Gk.-Arm. one.

In Hittite an IE combination of a lost laryngeal **H* and a reduced vowel *$_{e/o}$[15] results in *i-* in *innar-:innar-aw-atar* (strength, force, vigour), *innar-aw-ant-* (strong, forceful, vigorous, sexually potent): CLuw. *annar-* in *annari-* (forcefulness, virility), *annar-ummi-* (forceful, virile) with a prothetic vowel of a type similar to Gk. ἀνήρ, Arm. *ayr* (man).

6. IE *-ə- (a combination of a **H* with a reduced vowel) disappears in Hittite in the middle of the word: OHit. *zi-ke-et* (Zalpa story), 3 p.sg. past of *zi(k)k-*, iterative of *dai-* (to put), **dhH-sk'e-t.*[16] IE *-ə- (a combination of a **H* with a reduced vowel) > SA **-a-* in the middle of a word: HLuw. *tú-wa/i-tara/i-* (daughter), Lyc. *kbatr-a*[17] < IE **dhug'hH(*$_{e/o}$*)ter*, Skt. *duhitar-*, Gk. θυγάτηρ, Toch. B *tkācer*, oblique *tkātār*, A *ckācar*.

7. In Hittite IE **H-* has been dropped in the initial syllable of reduplication : Hit. *išḫiya-*(to bind) (after the loss of most reduplicated verbs in Hittite, this one might have been interpreted as not having reduplication at all). But IE **H-* has been preserved in the initial syllable of reduplication in CLuw. *ḫišḫiya-*(to bind), HLuw. *hi -sà-hi-* (the same type is repeated in other Luwian reduplicated verbs that formed a large and productive class).

8. In words for *good*, old phonemic variations of the beginning of a root may be supposed: IE$^{*}{}_{o}$*s-u-* (good) > Hit. *ašš-u-* (Skt. *su-*, cf. Gk. ἐυ–) CLuw. *wāššu-*, HLuw. *wa/i-su* (Skt. *vasu-*),[18] but see on the suffix in this word below, 12.

Morphological Isoglosses

Derivational Suffixes

9. Among significant isoglosses in the *nominal morphology*, one should pay attention to the change of the IE name for *earth* (originally a root noun in -**m/-n: *dh*[*e*]*g'hom* > Hit. *tegan/tagn-/tgan*, Toch. A *tkaṃ* [*tkan*],[19] Gk. χθών with a metathesis in the initial group of stop consonants). It transformed into an *-i-* stem in CLuw. *tiyammi-*[20] < **dheg'om-i-* : Bal. **g'em-yā/ē* > Lith. *žẽme*; Proto-Sl. **zem-i-/-y-ā/ē* > OCS *zemlya*, Rus. *zeml'a/na-zem'* (to the ground).[21] It seems possible to see the function of the suffix *-i-* in Luw. *tiyam-i-* as different from Luw. *tiyamm-ant-i-š* (the earth as magical substance in a ritual)[22] and the stem of a possessive adjective *tiyamm-ašši-š* (belonging to the sky). The form *tiyamm-i-* in a Luwian mythological narrative about the banquet of All the Gods[23] corresponds to Hit. *Dagan-zipa-* (the Divinity of Earth, the Spirit of Earth).[24] It occurs also in a mixed Hittite-Luwian mythological and ritual context in which an epithet—Luw. *dakkui*, Hit. *dankui* (dark)—refers to this divine creature.[25] The addition of the suffix *-i-* was connected to this symbolic representation of the earth as a divinity.

The element *-i-* in such stems as Hit. *warḫui-* (rough, shaggy) is often analyzed as a parallel to the *Motion* in the other IE dialects.[26] In Northern and Southern Anatolian its function need not necessarily be connected to the grammatical notion of a feminine gender that did not exist either in Hittite or in Luwian. But although in several cases where the suffix *-i-* is added to Luwian nouns denoting females, it cannot be proven that it has the meaning of gender. Still, one may think of a difference between Hittite, in which the epithet *dankui-* (dark) has been added to the neutral gender form *tekan*, and Luwian, where both the corresponding forms of an animate gender had a suffix *-i-*; cf. also 36 below for this suffix in such designations of females as HLuw. *tu-wal i-taral i-* (daughter) (but see on Lyc. *kbatr-a* above, 6); CLuw. *nana-šri* (*ya*) ([of] a sister).

10. Hit. *ḫaštai-* (bone): Skt. *ásthi-*, Sl. **kostĭ* < IE **Hos-t*(*h*)*-i* has a morphological structure different from Luw. *ḫa-*(*a-*)*aš-ša*, Lat. *oss* > *os* where no suffix *-t*(*H*) is to be reconstructed.[27]

11. Hit. *wett-* (year) < IE **wet-* is an athematic root noun while CLuw. *ušša-* (year), HLuw. *u-sal i-*, Lyc. *uhel i-*, *uha*(*-zata-*) (yearly [tribute]) < IE **w*(*e*)*t-so-* > Skt. *vat-sa-*.[28]

12. Hittite adjectives having correspondences in the other IE dialects show statistical predominance of the suffix *-u-* that is not as widely used in Luwian: Hit. *park-u-* (< IE **bhr̥g'h-u-* [high]: Arm. *bardzr*) corresponds to the stem in *-i-* of Luw. *parr-ay*(*a*)*-* (high) (cf. the Hit. verb *park-/park-iya-* (to rise, grow): Toch. *pärk-*), Av. *bərəzi-čaxra-* (the one [woman] to whom high wheels belong = related to the high wheels).[29]

According to Gordon Whittaker's hypothesis, a "Euphratic" IE dialect, some elements of which are found in the oldest Mesopotamian tablets in the fourth and third millenium B.C., contained a whole group of adjectives in **-u*[30] – like Euphratic

(Euph.) [*guru-d*] (thick, heavy) (Skt. *gur-ú-* [heavy], Gk. βαρ-υ-ς [heavy, thick]), that might be compared to the large number of archaic Hittite adjectives of the same type.[31]

Luwian differs both from Hittite and reconstructed Euphratic in this respect. Still, there is a small group of *-u-* adjectives common to several Indo-European dialects and probably going back to Proto-Indo-European that are represented in Luwian. To this group belongs a Cuneiform Luwian adjective *wāš-u-* (good) corresponding to Pal. *waš-u-* (good), Skr *vásu-*, Av. *vohu-* (good), Gaulish *Vesu-*, Germanic *Wisu-*; this stem was present both in Cuneiform Luwian and Hieroglyphic Luwian (*wa/i-su*, see above, 8), but not in Hittite (that had *ašš-u-* corresponding to Skr *su-*, cf. Gk. εὐ-).[32] The stem with the initial *w-* is present in NA Palaic that in some other isoglosses also joins SA Luwian.

Although there are few archaic adjectives like Luw. *wāš-u-*, *aru-* (high),[33] and several forms in *-u-i-* like *dakku-i-* (see above, 9), the bulk of comparable Luwian adjectives nevertheless belongs to the type in *-i-* like *parr-i-* (to them Luw. *zariya-* [rapid][34] belongs).

STEMS/WORD FORMATION

13. Semantic *type of compounding* in Luwian at least in some cases is different from that in Hittite. Luw. *aš- tumant-*, literally *mouth* + *ear*,[35] seems to represent a juxtaposition of two names of parts of a body similar to Toch. A type *ak-mal* (eye + cheek = face).[36] Favorite types of Hittite compounds are seen in OHit. *men-a-ḫḫand-a* (in front of), OHit. place name *Šall-a-ḫšuw-a* from a compound *belonging to a great king* < IE type *nominal stem* + **o* + *nominal stem;* Hit. *pattar-palḫi-*, name of a bird, lit. *wing/feather* + *broad* (noun + adjective in *-i-*), see above, 12. See also below (36–37) on Luwian compounds with *aš*(*a*)*r-* (woman) as a second part.

14. In the sphere of *nominal derivation* Luwian possessive adjectives in *-šši-* < -**sk-* of the same type as in Tocharian and Slavic (cf. Bal. adjectives in *-is/ška-* of the type of the OP *tawiskan* [*väterliche*])[37] occur in the function of Hittite possessive genitives. Luwian uses these forms in cases when in Hittite one may expect mostly genitives in -(*a*)*š* and *-an*.[38]

15. The Hittite *nominal paradigm* of plural is based on the same stem as that of singular. The Luwian nominal paradigm of plurals is based on the stem with the suffix *-n-z-* (< **n-s-*). A class of Luwian nouns (names of the parts of body: eyes, hands, feet) form "distributive plural" in *-nt-* (coincides with Tocharian class which forms plurals in *-nt-*: Toch. B *eśaive-nta* [eyes]).

16. There are no special forms of the locative plural in Hittite: the meaning is rendered by a dative (-loc.) plural in *-aš*. One may suppose that IE dialect loc. pl. **-s-u* (Skt. *-su*, Av. *-su/-hu/-šu*, B-S -**su*) has been preserved (maybe in another function) in HLuw. *tara/i-su* (thrice), Milyan (Mil. = Lyc. B) *trisu* (cf. Skt. *tri-ṣu* = Lith. dial. *trisù* = OCS *trĭxŭ*), Mil. *tbisu*, Lyc. *kbihu* (twice).[39]

17. Hittite quasi-ergative in *-nt-* represents a form used when necessary to

transform a noun of neutral (inanimate) gender into a noun of animate gender. A comparable role of an "activizer" may be attributed to Luwian quasi-ergative in *-šal-za*. The rise of quasi-ergative forms in Anatolian languages might have been stimulated by the ergative structure of several non-IE languages of the same territory. It is remarkable that in the (probably Middle Hit., if not OHit.) translation of a Hurrian (Hur.) mythological story, Hurian ergative *ta-a-re-eš* (the fire) is rendered by a Hittite quasi-ergative form *pa-aḫ-ḫu-e-na-an-za*.[40] In Luwian a similar function is performed by a nominal form in *-šal-za*: it changes the grammatical category of the noun that belongs to the neutral gender (animated neutral gender), and makes it possible to use it as a subject of an active construction (for instance, with the active verb *i-* [to go]). It is probable that these two formations are similar. But if they were due to the later influences, they might not belong to Proto-Anatolian as it is sometimes suggested.[41] At the same time, the second form seems to be very old: a comparison of Luwian *ašḫar-ša* ([by means of] blood) and Toch. B perlative *yasārsa* (with blood) sets one thinking about a continuation and transformation of an ancient grammatical tendency. Starting with Schuchardt and Pedersen, several great scholars of the beginning of the twentieth century were thinking of possible traces of an archaic IE ergative structure; some of these ideas were summed up recently by the late G. A. Klimov and other scholars also developing syntactic typological insights of Sapir.[42] It cannot be excluded that this Tocharian-Luwian correspondence could be related to the other traces of the original ergative/active role of the IE case in -**s* (the coincidence of the nominative singular and genitive singular in Hittite also was attributed to the same sphere of grammatical relations). But in Luwian the traces of the ancient system might be revived much later due to the new linguistic environment (a formal similarity of IE -**s* and of the Hurro-Urartean ergative case[43] has been remarked by Hrozný and Pedersen almost a century ago).

18. The Hittite form of a quasi-ergative in *-nt-* is related to the Hittite participle in *-nt-* as well as to the type of Hittite nouns like *gimm-ant-* (winter) (= Skt. *hem-anta-* [winter]). Although similar formations in *-nt-* have been preserved in Luwian, the value is different. Hit. *-nt-* participles are active only if derived from intransitive verbs and passive if derived from transitive verbs. The older situation has been reflected in the archaic double meaning: *adant-* (eaten / eating) (= IE dialect. *$_o$*dent-* [tooth]), *akuwant-* (drunk / drinking). Luwian has preserved a mediopassive participle in *-m-* (similar to that in B-S), and in it the *-nt-* participle has only the archaic active meaning.

19. Hittite infinitives in *-atar*, *-awar* going back to the heteroclitic nouns on *-r* are different from the Luwian infinitive in *-una* that is related to *-n* forms of the heteroclitic paradigms, as is also the Hittite supine in *-wan*. The closest analogies may be seen in Tochrian absolutive forms in -**wor*.

20. In *verbal derivation* Hittite suffixes play a role corresponding to reduplication that has been preserved in Luwian but was disappearing in Hittite. Anatolian Hittite verbal stem with a nasal infix *barg-an-u-la* (should be elevated) (derived

from *park-u-* [high], see above, 12) is synonymous to CLuw. *aru(wa)ruwa-* (to lift [?], to be raised up, elevated), a reduplicated verb[44] from the stem *ar-u-* (high) (above, 12, cf. OIr. *ard* [high]). The grammatical use of reduplication was typical for Southern Anatolian verbal forms different from Northern Anatolian, see above (7) on *ḫišḫiya-*.

21. Luwian has preserved an athematic primary root verb *i-* (to go) with a whole (in Luwian not suppletive) paradigm; cf. Toch. A, Toch. B *i-*, Skt. *é-ti*, Av. *aēiti*, Gk. εἶμι, OLith. *eimi*; Goth. *iddja*, 1 p.sg. past (cf. Luw. *i-ha*), Lat. *eo*, 1 p.sg. present. In Hittite in the same function an old combination of a cognate verb with a preverb is used. In Hit. *pai-* (to go), a former archaic IE preverb **pe* has been fused with an athematic root verb *(*e*)*i* > Hit. *i-* (a trace of it is preserved only as a suppletive 2 p.sg. imp. *it* < **i-dhi*; cf. Sl. **i-di* and the Sl. stem **id-* that had substituted the primary verb). From the same IE root a durative verb with a nasal infix (Hit. *iyannai-* : Lat. *-ī-n-unt*, Lith. *einù*) had been derived.

Hit. *uwa-* (to come), an old combination of the same root verb with the preverb *uw-*, corresponds to Luw. *aw-i-* (to come). Besides the root verb no correlative verbs derived from this stem are attested in the known Cuneiform Luwian texts, but a Hieroglyphic Luwian verb *pa-* (to go, to live) has been compared to Hit. *pai-*. It is supposed that the Hittite spatial opposition of the two prefixes (still preserved in Rus. *po-j-ti* [to go] : *u-j-ti* to go away]) is archaic; the meaning *to come* can go back as far as Proto-Nostratic.[45]

22. Differences in *verbal inflection* are numerous. Particularly important from a general IE point of view are the following Luwian endings different from corresponding Hittite ones: Hit. *-mi*, Pal. *-mi*, an IE primary ending of the 1 p.sg. of the present of old athematic verbs, corresponds to Luw. *-wi*, ending of the 1 p.sg. present; cf. Lyd. *-v* (1 p.sg. present), Toch. A *-we* (1 p.sg. past and imperf mediopass). A comparable correspondence of *-m-/-w-* is seen also in other verbal and pronominal forms of plural and dual and may be partly explained by reconstruction of IE inclusive/exclusive forms.[46] The necessity of morphological (and not phonemic) explanation of the difference is supported by different distribution of *-w-/-m-* in the other endings.

Hit. *-wen*, secondary ending of the 1 p.pl. past, has *-w-* (and not *-m-* as in *-m-i*) different from HLuw. *-min*, secondary ending of the 1 p.pl. past. Hittite endings of the 1 p.sg. *-ḫḫ-un* (past) and *-ḫḫ-i* < OHit. *-ḫḫ-e* (present) of the *-ḫi* conjugation correspond to the Luwian ending of the 1 p.sg. *-ha* in the past of the verbs of all classes[47] (IE dialect. **-a* as the ending of the 1 p.sg. perfect); Palaic is archaic in this respect and joins Southern Anatolian.

Hittite ending *-ir*, 3 p.pl. past (corresponds to an ending of perfect in Indo-Iranian and Latin), differs from Luw. *-nta*, a (secondary) ending of the 3 p.pl. past.

23. *LEXICAL INDEX OF THE PAST.* One may suppose that the whole system of forms describing the past in Luwian was different from that in Hittite. Particularly revealing in this respect is a lexical index of a past tense present in CLuw. *puwa* (for-

merly) (cf. probably also related verbs: HLuw. *pu-* and Lyc. *puwe-*).[48] The Luwian form is important both for *verbal morphology and as a vocabulary item*. It is found in such formulae as *pa-aš/u-na-at-ti-iš pu-ú-wa* [*ku-w*]*a-ti a-aš-ta na-nu-un-ha-aš a-pa-ti a-aš-du/a- a-ru* (and as she/the woman [during whose labor the rite has to be performed] was [*āšt*(*a*), 3 p.sg. past of *aš* < **es-*] *previously*, so [in a similar state] let her be/let her do now);[49] a comparable opposition of the activated neutral quasi-ergative form in *-za* of derived nouns *puwa-til-za* (the past) and *na-nun-tarr-i-* (the present) is attested in a ritualistic enumeration of binary oppositions (like *life* and *death*)[50] while in a partly similar enumeration[51] *puwa-til/puwala* (the past) is opposed to *pariyan-al-* (the future). Luw. *puwa* goes back to IE **bhuH-*, a suppletive verb *to be* from which the forms of the perfect (and modal categories linked to it including future) and/or the past and infinitive are derived in Indo-Iranian, Italic, Celtic, Germanic and Balto-Slavic: Lith. *bùvo* (it was, they were) = OIr. *bā*,[52] Sl. *by-lŭ* (past part.), Rus. *byl* (3 p.sg. past masc. [fem. *byl-a*, pl. *byl-i*]), *byloe* (past) (cf. Luw. *puwala* [past], cf. OP *boul-ai*), *o-byva-tel'* (inhabitant, resident) < *(*o-*)-*buH-VH-t*(*e*)*l-i-s* formally comparable to Luw. *puw-a-til*. In a Luwian combination *puwa ašta* (formerly he/she/it was) the semantic value of the past is coded twice: in the lexical meaning of *puwa* (cf. typologically Rus. *byvalo* [used to be]) and in the morphological meaning of the secondary ending *-t*(*a*), cf. Latin *fu-it*. One may suppose that *puwa* goes back to **bhuHe/o/*$_{e/o}$ = *bhuə* (it so happened) (see on *ə in Luwian above, 6).

In Hittite in a similar function *karu* (formerly),[53] *karuili-* (*former*, *previous*, *ancient*, e.g., about an old generation of gods in theogeny) are used (for instance, in Hittite laws and in some legal or ritualistic documents) as lexical indexes of a past event, cf. Gk. πάρος (formerly) in an archaic construction with injunctive: πάρος . . . ούτι θαμίζεις (you never used to go formerly) (*Σ* 86). In these constructions an archaic lexical way of expressing temporal (tense) correlations has been supposed.[54] According to Strunk (developing Wackernagel's interpretation of Greek and Indo-Iranian constructions), this function of *karu* goes back to Proto-Indo-European. HLuw. *rú-wa/i-na* (*formerly*,[55] a NA borrowing?) goes back to **k'ruw-an* = **gruwan* (Hit. *karu*) with a loss of the initial (voiced or glottalized?) stop. A verbal stem **bhuH-* has not been attested in Northern Anatolian. According to Meriggi's interpretation of a Lydian form of the 3 p.sg. pl. past *e-l* < **es-l* (he/she/it/they was/were)[56] it has the same meaning and ending as a modern Russian form *by-l* (he was) (cf. OP *boul-ai*), but the difference between two suppletive stems of **es-*/**bhuH-* is not relevant for this NA dialect. A similar conclusion for the whole NA-Toch. dialectal area can be reached on the base of the equation of Hit. 1 p.sg. voluntative *eš-l-i-t/eš-l-u-t/aš-all-u*, Toch. A *nasäl* < **no-es-l-*, and Toch. B *nesalle*.[57] If OLith. *esle* is related to the latter two, it may be supposed that Eastern Baltic (similar to Hit. and Lyd., i.e. NA, and Toch.) could use the stem **es-* in a combination with a modal ending -**l-*, different from Western Baltic where as in Slavic a suppletive stem **bhuH-* had to be used: OP *bou-lai*, OCS *bylŭ*.

Hittite was peculiar in the absolute absence of any trace of grammatical role of **bhuH-*. This negative isogloss unites it with Tocharian, Albanian, and Greek-Armenian[58] as different from Aryan, which can be grouped together with Southern Anatolian, Northwestern Indo-European, and Western Indo-European from this point of view. It is remarkable that the area of the use of **bhuH-* as a suppletive stem includes the two more-narrow territories marked above: that of satem languages (with the exception of Armenian and Albanian) and a zone with a specific form of the locative plural in **-su*.

Negation

24. As shown by Starke,[59] privative syllabic **n̥- > a-*: Hit. *ammiy-ant-* (small) (cf. Pal. *ūmmaya-* [immature])[60] corresponds to caritative Luw. **ne- > ni-*: *ni-waral-lai-* (alien, hostile), HLuw. *na-wa+ra/i-li-*.

Syntactic Isoglosses

25. Hit. prohibitive *le* differs from prohibitive Luw. *niš*, HLuw. *ni(s)* and Lyc. *ni*.

26. Hit. enclitic conjuction *-(y)a* (and) (Toch. A *-yo* [and]) < a stem of a relative pronoun **yo-* (cf. a functional typological parallel in Lat. *-que*: *quis*) is different from Luw. enclitic conjuction *-ha* (and).

27. Hit. *katta* (under, below) (borrowed into HLuv.?), probably having an areal connection to Gk. κατά is synonymous with Luw. *ānnan* (under, below), Lyc. *ęnę*.

Universal quantifier

28. Hit. *humant-* (all) (cf. Lat. *omn-is*?) corresponds in quasi-biligual texts to Luw. *punata/i* [all]: Toch. *pont-*, Gk. παντ-.[61]

Lexical isoglosses

In details mass lexical differences between Hittite and Luwian in their basic vocabularies are represented in tables 1 (a, b)–3. The following lexical items might be discussed separately:

Material Culture

29. *WHEELED TRANSPORT.* A large zone including all the IE dialects except Northern Anatolian is defined by a name of a wheeled vehicle, *wheel* derived from a root **kʷel-* (to turn). In "Euphratic" (*gigira* [chariot, wagon] known through Sumerian),[62] Toch. (A *kukäl*, B *kokale* [carriage]), Eastern IE (Indo-Iranian, cf. Skt. *cakra-*; Mycenaean and Homeric Gk.; Phrygian) and Germanic a perfect type of reduplication of this stem is used. In HLuw. *zalal-* (chariot, vehicle, cart) derived (with later palatalization of a labiovelar in a position before a frontal vowel) from the

root of Luw. *kuwal-* (to turn), there is a probable trace of an older (partial?) reduplication. CLuw. *zal-war/n-* (riding in a chariot) derives from the same root without reduplication as cognate words in Old Prussian, Slavic, and Celtic do.[63] Again the dialectal area for a non-reduplicated term is relatively narrow and coincides with the isoglosses enumerated above.

Although in Hittite some traces of derivatives from *ku*(*e*)*l-* are found, the main verb in the meaning (to turn) is different from the Luwian one: Hit. *weh-* is used particularly in descriptions of the training exercises for horses in hippological treatises. A name for chariot derived from the root **k*^{w}*el-* is absent in Hittite.

30. While speaking of riding vehicles, Hittite texts use the verb Hit. *iya-* (to ride, to move in a vehicle) (a derived mediopassive verb from the lost verb *i-*, see above): Skt. *yā-*, Toch. A *yā-* : Lith. *jójo*, OCS *ja/ěxati*.[64] In Southern Anatolian another IE root is used in the same meaning: HLuw. *wiza-* (drive), a form derived with the suffix *-za-* < **sk'e/o-* from IE **weg'h-*; cf. derived nouns: Skt. *vāhana-*, Myc. Gk. *wo-ka*. As it can be seen from the OP compound *kelle-wesze* (driver),[65] a non-reduplicated form of the name for wheeled vehicle was used together with the verb **weg'h-* (to drive), attested in verbal derivatives in Southern Anatolian, Balto-Slavic, and Western Indo-European, while the area of the nominal formations derived from it and denoting different notions connected to wheeled transport and horse training extends to all the ancient Indo-European dialects except Hittite.

31. Two last lexical items might be interesting for an attempt to relate dialectological connections to possible ways of migrations for which wheeled transport was indispensible. Hittite and Tocharian have another term for a wheeled vehicle different from the one discussed above: Hit. *ḫuluganni-* (chariot), a probable old Hurrian borrowing; cf. another related term maybe borrowed by IE from N-Caucasian: Hit. *ḫurki-* (wheel): Toch. A *wärkant-*, Toch. B *yerkwantai* (wheel), a possible satem correspondence in HLuw. $^{\text{CURRUS}}$*wa/i-ra/i-za-ni/nī/ná/na* (chariot).[66] It might have referred to a common type of vehicles used by both groups of Anatolian Indo-Europeans and Proto-Tocharians as well as by Hurrians and other tribes speaking Northern Caucasian dialects. The term may describe the objects used close to historical times when these IE and non-IE languages formed one linguistic league, while the words studied in the two preceding paragraphs were probably introduced at the period of the common migrations of the rest of Indo-Europeans.

32. The importance of Hurrian terms for the part of vocabulary designating movements of people and vehicles is corroborated by the Luw. *ḫar*(*u*)*wa-* (road); a probable Hurrian borrowing: Hur. *ḫari-* (road). Hittite uses in the same meaning Hit. *palša-* (road) (HLuw. *pal*[*a*]*sa-*?);[67] cf. also Hit. *itar* (going) (from the lost *i-* [to go]; see above): Toch. A *ytar*, Lat. *iter* (road).

Social and Military Vocabulary

33. Luw. *ḫanta-wat*(*i*)-(supreme authority, king, Lyc. *χnta- wat*(*i*)- (ruler, king) can be traced back to IE **Hn̥t-o-wodh-* (front- + lead-er), comparable to B-S names

of a *leader*: Lith. *vadas* (leader), *pra-vadas* (leader) (cf. OPr *pra-weedd-uns* = *durchgeführt*), *vedėjas*, OCS *voždь*, *predь-voditelь*, *voje-voda* (general) (cf. the semantic type of Myc. *ra-wa-ke-ta* = λαFαγέτας). The terms shared remarkable coincidence in the use of the verb **wedh-* (to lead) in a specific meaning associated not to the widely represented general IE (and also B-S) carry-off-a-woman-by-force marriage custom,[68] but to the role of a leader. In NA Hit. *ḫaššu-* (king) (attested already in Ebla archives of the third millennium B.C. in the name of a city Ḫazzuwa, cf. OHit. *Šall-a-ḫšuwa* (Of the Great King [a city name]) is usually identified to Skt. *asu-ra-* (lord, god [Av. *ahura-*]) and an ON name of gods. In Southern Anatolian as also in Balto-Slavic such an archaic term might have been substituted by a descriptive compound.[69] As in some other cases discussed above, the area to which both Southern Anatolian and Balto-Slavic belong seems to be an innovative central one. If (as it might be supposed on the basis of the terms for wheeled transport) speakers of some northwest IE (particularly Proto-Balto-Slavic) and Proto-Southern Anatolian dialects were moving together, at some stage of their migrations they might have had a unified social organization.

34. As discussed above, the term for *king* or *leader* may refer to the person leading the army; thus the word designating *army* becomes important. Hit. *tuzzi-* (army) was probably related to a Western IE word for *people*, *tribe*: Lith. *tautà* (people). Luw. *kuwala-* (army) was probaly connected to the stem of Slavic **čel-ędĭ* < **k*^{w}*el-endi* (servants [in a feudal estate]).[70]

35. To social and military terms shared by Germanic-Balto-Slavic and Southern Anatolian probably one should add Luw. *wal-* (to die), different from Hit. *ak-* (to die = to be killed)[71] and OHit. *mer-* (disappear = die [in the Zalpa story]): IE dialectal (also NW IE) **mer-* (to die). The Luwian verb is cognate to Toch. A *wäl-/wal-* (to die). In this case the area includes not only Southern Anatolian and Northwestern IE[72] but also Tocharian. It seems important that in this area both specific expressions for death and birth are found that differ from those in the other dialects.[73]

TERMS FOR FEMALES

36. Besides the common IE (and ancient Nostratic/Eurasian) term for a *woman*, *wife* (Hit. *kuinna* [woman, wife?],[74] Lyd. *kāna*, Luw. *wānā-* (woman) < IE **g*^{w}*on-e/oH*) in Luwian there was a word *ašr(a/i)-* < **e/os(o)r-* (female, woman), a noun deduced from *ašra-ḫit-ašši-n* (belonging to womanhood) (a loan translation of a Hurrian expression *ašt-ašḫi* [womanhood] with a Luwian abstract suffix *-ḫit-*),[75] *ašr-uli-* (female) (a rendering of a Hurrian expression *ašt-uḫḫi* [female]), *ašr-ula-ḫit-* (femininity). It seems that this IE stem has been preserved in this way only in Luwian and Homeric Greek ὄαρ, with an initial vowel lost in the rest of IE dialects (Indo-Iranian *str-ī-* < **sr-iH-* shows later changes of the stem after the addition of a suffix *-*iH-* > *-ī-*). Probably at the time of Hurrian-Luwian bilingualism in the south of Asia Minor, Hurrian influence had helped to preserve the word as an equivalent of a Hurrian term for the principle of *womanhood*.[76] In Hittite this morph (that had

lost an initial vowel) already has a derivational status: Hit. *-šara-*, a suffix of forms like *ḫaššu-šara-* (queen) that probably go back to compounds with the second element **-sor*; cf. the type of IE **sw-esor*[77] (sister = woman of [one's] clan) and the suffix of the same origin in the feminine forms of numerals *3* and *4* in Indo-Iranian and Celtic.

37. In some derivatives having this second element, the suffix *-i-* is added to it. This suffix is comparable to the *-ī* < **iH-* morph of a collective > IE dial. feminine (above, 9): Luw. *nan-ašri* (*ya*) ([of] a sister) is an old compound with the same structure and the same second stem as in IE **sw-esor*, cf. above on Luw. *ašra/i-* and Indo-Iranian *str-ī-*. In this Luwian form the morph **-asr-* (+ *-i-*) is added to a derivative from the stem of Hit. *nega-* (sister), an archaic Nostratic term[78] : Hit. *neg-na-* (brother). Although Luw. *nana-* (brother) seems to belong to the type of the kinship terms of *Kindersprache*, its shape may be a result of secondary phonetic development; cf. Hit. *negna-* (brother) < **neg'h-no-* > Luw. **ne-no-* > *nana-*.

Religious Vocabulary

38. OHit. *šiu-* (god), Hit. *šiu-na/i-* (god) differs from Luw. *maššana/i-* (god), Lyc. *mahana-* (god) (also Carian and other Southern Anatolian names with this element). The IE root from which the Hittite noun had been derived is also present in Hit. *šiwatt-* (day) (etymologically = Luw. *Tiwatt-* [Sun God]) (Lat. *dies* : *deus*). The Hittite name of day is lexically different from Luw. *hal-li-ya-* (day) : HLuw. *ha-li*(*-i*).[79]

To some extent all the abstract and some concrete notions (including some words discussed above) might have acquired religious values.

39. Hit. *kunna* (on the right hand, favorable) as one of the central ritualistic terms denoting a positive member of a dualistic (binary) opposition (especially in the oracular texts and in descriptions of ritualistic performances) is different from Luw. *i-šar-*(*ú-*)*wa-ya* (*-an*) (favorable, right) and *i-šar-ú-i-* (*/*wi_5*-*)*-li-iš* (right [hand]; favorable).[80] Luwian adjectives constitute an exact match to the OP stem *isarwi-* [*izarwi-*] (true, real, faithful). Semantically the meaning *favorable* or *right* is very close to *true* (as can be shown, by the way, on the example of modern Eng. *right* that can combine both these senses).

From the Luwian rituals in which these adjectives are used[81] and from the other Luwian and Hittite texts, it can be seen that evil was associated with the left side. The right side was that of luck.[82]

40. In Hittite mythological texts one meets a personified name of Hit. *naḫšaratt-* (fear) (an abstract noun based on the root of Hit. *naḫḫ-* [to be afraid], Luw. *naḫḫuwa-*). In a mythological context Hit. *naḫšaratt-eš weritem-eš* ([personifications of] Fear and Terror) render Hattic *tauwa*$_a$ *tupi* (with N-Caucasian parallels)[83] = Gk. *Δεῖμοστε Φόβοστε*. In Southern Anatolian the word is different: HLuw. *kwaya-* (fear), CLuw. *kuwayat-* (fear) (the same type of an abstract noun from another root: IE **dwei-* with a Luw. change **dw* > *kw-*; semantic parallels are known in derivatives of this root in Greek and Armenian).

41. Hit. *mehu-r/-n-* (time) is cognate to IE **meH-* (to measure) and to dial. IE names of *moon, month, year* (Lith. *mẽtas,* Alb. *mot* [time]) derived from it. Luwian has another term. CLuw. *āra/i-* (time), HLuw. *a+ra/i* (time)[84] is related to Gk. *ὥρα* (a definite period of time, season), Goth. *jer* (year), Sl. **jarŭ* (spring, beginning of the year), Rus. *Jar-ilo* (paganistic god of the spring and fecundity), *jarovoj* (spring-sown [seeds, grain]).

Because many Hittite and Luwian ritual texts describe magical (shamanistic) operations on a sick person's body parts, it is possible to connect large changes in this part of an inherited vocabulary—particularly in the case of Luwian—to the corresponding religious ideas and taboos.

42. Hit. *ḫaršar* (head) that continues an old IE term[85] is different from CLuw. *ḫarmaḫi* (head), HLuw. (CAPUT)*ha+ra/i-ma-* (head = person).[86]

43. A taboo distortion of a common borrowing from a non-IE source may be present in Hit. *tetana-* (hair), Luw. *tappani* (hair).

Wine and Vine

44. Hit. *wiyana-* (wine) is semantically different from cognate HLuw. *wiyani-* (vine). In several HLuw. ritual texts the god called Tarhunt (Storm God) of the Vineyard (*tu-wa/i+ra/i-sà-sa [DEUS]TONITRUS-hu-za-sa-*) is celebrated. Theophany described in these texts coincides with the sculptural images of the god representing him with bunches of grapes that encircle his body.[87] It seems that the same cult in connection with vine is indicated in a CLuw. mythological fragment wherein, after the form *ú-i-ni-ya-du-wa* in one of the next lines, a broken verbal form *tar-ḫu-un-ta-at-ta* . . . derived from the name of this god occurs.[88] In rituals of Ĭštanuwa participants "drink the god of the Vineyard (or Wine?)" (*^{D}Ù-i-ni-ya-an - da-an a-ku-wa-an-zi*).[89] These texts were written in Hittite, but they represented the religion of a Luwian-speaking population. They may show how the meaning *wine* has developed from that of *vine*. The latter seems to be original in the whole group of IE, Afro-Asiatic, Kartvelian and Hattic terms related to these words.[90]

CLuw. and HLuw. have preserved the word **medhu-* > CLuw. *maddu-*, HLuw. *ma-tu-sa* meaning wine or alcoholic beverage (*mead* being attested in cognate words in Iranian, partly in Vedic and some later Indo-Aryan and Nuristani dialects, Mycenaen and Homeric Greek, Germanic, Slavic, and Celtic).[91] Hittite (as many other IE dialects) substituted the word by a derivative of the old name for *vine* preserved in Luwian. Thus the whole structure of the semantic field has been changed due to the absence of **medhu* in Hittite.

Water and Washing

45. Hit. *arra-* (to wash) presents an exceptional Northern Anatolian-Tocharian isogloss: Toch. A *yär-/yār* (to wash).[92] A semantic archaism probably connected to the absence of this term in Luwian is present in Luw. *ililḫa-* (to wash out), a reduplicated verbal stem, cf. Luw. *elḫa*(*i*)-, *la*(*ḫ*)*un*(*a*)*i-* (to wash),[93] Arm. *loganam* <

*_lowHnami_, Gk. λούω (bathe, wash), Lat. _lāvo_. In Hit. _laḫḫuwai-_ (to pour), Luw. _liluwa-_ (to pour) (reduplicated, with a loss of a laryngeal) there is a semantic shift probably due to the interference of a synonymous dialectal term.

46. Hit. _watar_ (water) (with a parallel form of a root noun _wid-_) presents a widely spread IE term. In Indo-Iranian it coexists with a nearly synonymous[94] term for (a portion of) water represented in Luw. _war_ (water) (Toch. A _wär,_ B _war_). A noun corresponding to the Hittite one is absent in Luwian.

47. Hit. _aruna-_ (sea; large salted lake) that may be connected to Skt. _arṇava-_ (sea) is different from Luw. _ala/i-_ (sea)[95] (cf. IE *_sal-_ [sea, salt]).[96] The importance of large lakes and seas for the geographical environment of Asia Minor makes this difference in vocabulary particularly relevant.

ADJECTIVES

Besides adjectives in _-u-_ discussed above (12), there are other important lexical differences in this word class between Southern and Northern Anatolian:

48. Hit. _daluki-_ (long) is cognate to Skt. _dīrgha-_, Gk. δολιχός, Sl. *_dŭlgŭ_ (Pol _dl·ugość_[97] = Hit. _dalugašti-_), Lith. _ilga-s,_ Lat. _longu-s_, Goth. _lagg-_ (long). In Luwian another word is attested: CLuw. _ārray(a)-_ (long), HLuw. _a-ra/i-_ (long). It is cognate to Toch. A _aryu_ (long). This is one of the exclusive S A–Toch. isoglosses. But a derivative deverbal _-nu-_ factitive verb Luw. _ara-n-u-_[98] is identical in its structure to a deverbal _-nu-_ factitive verb Hit. _dalug-n-u-_ (used in an archaic mythological context).

Probable Borrowings

Besides some possible borrowings tentatively pointed out above, the following differences between Northern and Southern Anatolian may be due to loanwords in at least one of the dialects (or in both of them from similar or different sources).

49. Hit. _šuppi-_ (pure), _parkui-_ (pure) belongs to a group of archaic adjectives in _(-u)i-_ (cf. above, 9, 12, 36, 38). A Luwian term with the same meaning, _ḫalali-_ (pure), was borrowed from (NW) Semitic.[99]

50. Hit. _antu-ḫš-a-_ (human being) is a compound (< _born by the people_, cf. OHit. _antu-_ in a possible meaning of _people_[100] in an OHit.-Hattic hymn praising the "king and the queen"). Cf. the compound with the same first part in Myc. Gk. _a-to-ro-qo_ > ἄνθροπος. Luw. _zita/i_ (man) (attested in a proper name in the archives of Ebla already in the middle of the third millennium B.C.) can be explained either from IE *_g_^{w}_ei-_ (to live)[101] (OLith. _gi-ti_ [to live], OCS _žiti_) or as an ancient N-Caucasian borrowing.

51. Hit. _tešha-/zašhi-_ (dream), a borrowing from North-Caucasian (Proto-Adyghe-Kabardian *_pčəħá_),[102] enters the same semantic field as the verbs _šup-_ (to fall asleep, to dream) < IE *_sw(e)p-_ and _šeš-_ (to sleep, to be quiet, to rest, to have sexual intercourse with) < IE *_ses-_.[103] The first part of the Luw. _duntariyašḫa_ (dream) may be related (if the initial phoneme is similar to that in _tawa_ [eyes], above, 2) to

Gk.ὄνειρον, Arm. *anowrdž*, Alb., *andërr* (dream, sleep), but the second part is not clear at all: is a coincidence of the end of the word with the Hit. *tešḫa-* due to a chance? Is it a compound?

52. Hit. *šall-i-* (great = royal) (+ *pir* [house > palace]; + *pedan* [place > throne], + *ḫaššatar* [royal kin]; OHit. place name *Šalla-ḫšuw-a* from a compound *belonging to a great king*, see above) comes from Common Semitic *šl-* (powerful).[104] CLuw. *ura/i-* = HLuw. *ura/i-* (great) might have influenced later Hittite official terminology of the Southern (Syrian) areas such as Ugarit, but there is no evidence for existence of a similar word in Hittite proper.[105]

53. Hit. *ištama-š-* (to hear): *ištama-na-* (ear) is derived from a non-IE stem with the suffixes similar to those in IE *k'lew-s-* / IE dial. *k'lew-n-* (as reflected, for instance, in Tocharian words for *to hear/ear*),[106] cf. Afro-Asiatic parallels: Ancient Egyptian *śḏm* (to hear), Semitic √*šm*, with a *-t-* stem; similar forms are known also in Kartvelian. CLuw. *tumman(t)-* (ear, hearing): *tumman(t)- iya-* (hear) = HLuw. *AUDIRE + MI- (ma)-ti*[107] seem to come from a similar non-IE source.

The lexical isoglosses studied above do not exhaust all possible items of this kind. Each of the different Hittite and Luwian words included in tables 1–3 might be discussed from the point of view of their relationship to the vocabularies of the other IE dialects or non-IE neighboring languages.

If branches of a linguistic family differ from the other branches in those features that can be explained as results of internal change in one dialect, those differences do not seem crucial for finding borders between dialects. In the list above more or less simple correspondences (such as Hit. *e*: CLuw. *a*, Hit. *-zi*: Luw. *-ti*, Hit. *ši-*: Luw. *ti*) are not mentioned since the undergoing changes (*e > a, *ti > zi,[108] *di > ši) are typologically frequent and do not necessarily point to great differences between dialects.[109] Still, even these relatively trivial and well-established (and enumerated in all the textbooks) correspondences (that would have added a number of items to the list given above) help to support the general feeling of an extremely large distance between these two linguistic groups.

To measure a distance between Hittite and Luwian, one might try to use (with some difficulties and approximations caused by the paucity of Luwian lexical materials) the standard technique of glottochronology. In table 1a correspondences between Hittite and Luwian in a 100-word list are given. Although many Luwian words (and some Hittite ones) belonging to this part of the basic vocabulary are not known, the difference between the two languages is found no less than in 15 percent of the items (11 out of 72). In table 1b the next 100 words of a 200-word usual glottochronological list (with some minor additions) are studied. In them the portion of items absent in Luwian is no less than 21.3 percent (20 out of 94). Summing up, in both the lists 31 words out of 166 are absent, which gives the measure of 18.7 percent. But as the proportion of unknown Luwian words in both the lists is very large, this measure indicates only the minimal difference. Accordingly, the index of preservation of the inherited basic vocabulary *m* is no more than 0.813 or 81.3 percent.

It is possible to find another measure of the difference of the inherited vocabularies of the two dialects. Not long ago Sergei Starostin suggested a new variant of "root glottochronology." A text in a given language is searched for words having reliable etymologies. For each of them a corresponding derivation in another language is found. The proportion of correspondences between the pair of languages does not depend on the genre of the texts but reflects their age.[110] Let us perform this procedure with the Old Hittite texts of the Inscription of the King Anitta[111] (rows 1–72 in the table 2) and the first part of the Story of Zalpa[112] (rows 73–101 in the same table). The proportion of clear roots shared between the two languages is about 0.38. But this figure indicates only the maximal difference. The index m of the preservation of the vocabulary is no less than 0.38. Thus, according to the measurements reflected in tables 1 and 2, the index of preservation is defined by the inequality: (1) $0.38 < m < 0.812$

But this definition is still not exact enough. Too many Luwian words might be absent simply because of the scarceness of texts and because of our insufficient knowledge of the vocabulary. To reduce difficulties connected to these factors, let us start with Luwian texts using Hittite as a dictionary language. Because in this case we start with a Luwian text, most of the Luwian words are known and the factor of the insufficient lexical knowledge of this language does not influence the results of our measurement. The rituals of Kuwatalla[113] are searched for words of Indo-European origin. As may be seen from table 3, the percent of preserved roots in Luwian is $m = 0.67$ (a result agreeing with the inequality 1 found by independent measurements). This measure of the difference between Luwian and Hittite is roughly the same as that found by Starostin (using the same technique of root glottochronology) between Ancient Greek and Latin (0.67), Ancient Greek and Vedic (O.69), Latin and Vedic (0.71), and Latin and Ancient Greek (0.72).[114] According to Starostin that indicates similarity of the distance of each of these ancient Indo-European languages from the common protolanguage. Relevance of these numerical data for the Indo-Hittite problem is beyond doubt. Luwian and Hittite are approximately at the same level of distance from the Proto-Indo-European (or Proto-Indo-Hittite) as are Latin, Vedic, and Greek: that is, there is no necessity to reconstruct separate protolanguages for these dialects. But the difference between Hittite and Luwian is quantitatively as large as that between the other ancient languages constituting separate branches of IE.

The difference between Hittite and Luwian vocabularies is reflected not only by paradigmatic study of the dictionary, but by syntagmatic investigation of roughly synonymous texts. When common mythological notions or plots were expressed in languages of these two groups, concrete word forms might be absolutely different: the same mythological motif might be called in Hit. *ḫumant-eš šiun-eš* (all the gods), in Luw. *-punatinzi maššaninzi*, in Lyc. *-mahai uwedri* (Mil. *masaiz uwedriz*). There are no identical morphs in these two formulae in Northern and Southern Anatolian. (If one compares them to semantically similar Mycenaean Greek and Sanskrit expressions, it is possible to find the same roots as Hit. *šiu-n-* : Skt. *deva-*; Luw. *punata-;* Gk. *παντ-*,[115] so in this case Greek and Luwian or Sanskrit and Hittite have more in common than Hittite and Luwian.)

At the same time, the first steps of Anatolian comparative grammar (as different from the history of the Hittite only)—as developed by P. Meriggi,[116] B. Rosenkranz,[117] and C. H. Melchert[118]—have shown a lot of similar and even identical features. The three tables in the supplement to the article not only show the lexical differences measured above, but at the same time indicate a number of similar or identical basic elements of the vocabulary. The general inventory of phonological and grammatical categories, schemes of declension and conjugation, and structure of sentence and discourse seem to be practically the same.

How can that be explained if we accept the idea of an initial difference between two branches of Anatolian?

First of all, many words, forms, and linguistic rules that are common to all the Anatolian languages or at least to the best known of them (Hittite and Cuneiform Luwian) can be traced back to their common Indo-European ancestor. The main relations between phonemes and morphemes and the bulk of the basic dictionary are Indo-European. The question arises: Is Proto-Anatolian (reconstructed on the basis not only of Hittite, but also by paying attention to the other less known languages) really significantly different from Proto-Indo-European?

The answer is negative.[119] Those features that from the beginning of comparative Hittite studies were considered to be most characteristic of Anatolian—such as laryngeals, heteroclitic declension, and peculiar verbal structure[120]—reflect first of all the features of a language that had been the common ancestor of Hittite, Luwian, and the other IE dialects. To make our discussion more fruitful by accepting terms used by other participants at the symposium, let us call this common ancestor "Proto-Indo-Hittite."[121] This reconstructed language should be quite different from the one described by Brugmann (although the number of brilliant insights just of this scholar that have been proved by Anatolian and Tocharian is amazing). Reconstructions are based on correspondences. The latter constitute the stuff of our science. Constructions built on their base might be different. But correspondences themselves remain. Because the number of correspondences has increased significantly, Proto-Indo-Hittite reconstructions should differ in their substance from those known before Hrozný and Forrer. I do not have in mind the idiosyncratic approaches to reconstruction of separate scholars. Differences between them do not reflect correspondences as a formal linguistic network. They refer to the metalinguistic level.[122]

From the metalinguistic point of view only isoglosses are real. We distinguish dialects combining isoglosses according to our operational technique. So it is possible to reconstruct a number of different protolanguages with different structure to account for a definite set of comparative correspondences. Sturtevant used the metalanguage of his "Indo-Hittite Laryngeals" to preserve Brugmann's IE reconstructions, adding to them a level of deeper Indo-Hittite reconstruction. In the same way it is possible to divide some interpretations of established correspondences between protolanguages (of a prewritten or prehistorical period) of each separate written Anatolian language: Proto-Anatolian and Proto-Indo-Hittite. But since the two latter systems should coincide, to economize in building proto-systems one may try to deal with Proto-Indo-Hittite, interpreting each correspondence between separate Anatolian languages and their relatives from the point of view of this metalinguistic construction.

To disprove the necessity of a special Proto-Anatolian stage in our interpretation of discovered data, one should show that features ascribed to Anatolian might refer to Proto-Indo-Hittite as well or to a group of its dialects larger than Anatolian. Thus it is right that in all Anatolian languages, an ending *-t-u* is found in the 3 p.sg. imperative.[123] But the same ending is found also in Indo-Iranian and probably in Phrygian: OHit. *e-eš-du* (that is, *let it be*, Tel., II 69), Pal. *a-aš-du*, Cluw *a-aš-du*, HLuw. *à-s-tu* (in the old inscriptions of the time of the Hittite Empire), Lyc. *esu;* Skt. *ástu*, Av. *astu*, cf. Phryg. ειτου.[124]

A group of the words of the basic vocabulary that is common to Anatolian languages and at the same time different from synonymous words in other IE dialects is shared also by Tocharian: Hit. *eku-* (to drink),[125] Pal. *aḫu-* (to drink), CLuw. *u-* (with the loss of **-k-/-g- > 0*), Toch. A *yok-;* Hit. *iya-* (to make, to do), CLuw. *aya-*, Lyc. *a-*, Toch. A *ya-*. Several archaic forms are especially similar only in these languages (e.g., Hit. *paḫḫur/paḫḫuwar* [fire], Toch. B *puwar,* Toch. A *por < *paur*); see above on Toch. Perlative. The recent results of IE linguistic geography and computational cladistics applied to it coincide in the suggestion that the separation of Tocharian and other IE dialects followed the break with Anatolian and preceded the dispersal of all the rest of IE. It may be supposed that Anatolian dialects were united with Proto-Tocharian by a net of isoglosses. For a certain period after the dispersal of "Indo-Hittite" a unity that included both Southern and Northern Anatolian and Tocharian may probably be postulated (see above on the terms for the wheeled vehicles). One linguistic feature might help in establishing a relative chronology of this unity. Among verbs common only to Southern and Northern Anatolian and Tocharian there was a dialectal IE term for *to give* different from **dō* < **doH-* used in most languages: Hit. *pai* (to give), Lyd. *bi-d-*, CLuw. *pija-*, HLuw. *pi-ya*, Lyc. *pije-*, Toch. B *ai-* (to give), Toch. A *e- < *ai-* (from a semantic point of view this lexical dialectal difference seems particularly significant if we take into consideration Mauss's theory of gift as "a total social fact"). In Luw. *pipi-šša-* and Lyc. *pibi(je)* the reduplication[126] includes the initial consonant of a former prefix that shows a relatively late character of this formation. The unification of this preverb with a root is similar to what is found in Hit. *pai-* (to go) as different from Luw. *i-* (see above).

It is possible that Tocharian enters together with Hittite and Luwian into a dialectal zone for which the use of an enclitic pronominal morph **-me/o-* in dat. pl. is characteristic: Hit. encl. *-š-maš* (to them), CLuw. *-mma-š*, Toch. B *-me*, Toch.

A *-m*. According to Sommer's idea, it is possible to compare this enclitic pronominal element to the pronominal and nominal endings containing **-m-* in Northwestern IE dialects.[127] If this hypothesis is correct, Northern Anatolian, Southern Anatolian, Tocharian, and an eastern subgroup Proto-Northwestern-Indo-European (Balto-Slavic-Germanic) dialects belonged to one area at the time when the pronominal/nominal paradigm of the IE Plural had been shaped.

One of the main problems of the modern theory of reconstruction is that of relative chronology. Not all the features found in a group of related languages spoken for a long period in the neighboring areas should be old. A reevaluation of an older IE device might be seen not only in the quasi-ergative Hittite and Luwian forms discussed above, but also in the chains of enclitics that are characteristic for all IE languages of Anatolia. The initial stage of their development may be seen in the earliest IE sentence structure (discovered also in some relatively recent studies on Mycenaen Greek, Novgorod birch-bark documents dialect, etc.),[128] and thus this feature is not at all specific for Anatolian.[129] At the same time as mentioned by some scholars, several enclitics as well as syntactic elements introducing the whole chain of enclitics are different in Northern and Southern Anatolian (and even different at the several stages of the history of one language: OHit. *ša* and *ta* were absent in the next period). Among non-IE languages of the same linguistic zone, Hurrian has typologically similar grammatical elements called "associatives" by Speiser in his Hurrian grammar. Such a Hurrian enclitic as *-an* referring to the direct speech is equivalent in its function to a quotative particle: Hit. *-wa(r)*, Luw. *-wa* (related to the stem of Pal. athematic verb *wer- ti*, Hit. *wer-iya*: Gk. *εἴρω*). Although all Anatolian dialects might have continued a general IE tendency to put a complex of enclitics in Wackernagel's position after the first (stressed) word of a sentence, a later development of this feature in each separate language might have been influenced by neighboring dialects. That is a typical situation of a linguistic league.[130]

Among the culturally important specific words common to different Anatolian languages, particularly interesting is the noun *house* : Hit. *pir,* oblique *parn-* (*šall-i pir* [great = royal house, palace]); Lyd. *bira-*, CLuw. *parna-*, HLuw. *parna-*, Lyc. *prñna-wa* ([grave-] house, mausoleum), *prñnawa-* (build, construct), *prñnezel i-* (household), *prñnezi(je)* (household member). It has been borrowed in the prewritten period from Ancient Egyptian *pr* (house) (> Coptic *-ΠΩΡ*, *-ΠΕΡ*). In Hittite it formed an archaic heteroclitic declentional paradigm showing an old type of the vowel alternation and an oblique stem in *-n-*. The latter form spread in Southern Anatolian languages, while in Lydian a form morphologically similar to the Egyptian source and to OHit. nom.-acc.n. has been preserved (although it suffered phonetic change of the initial **p- > b-*). The word spread in the large area of a whole linguistic league as seen from Hur. *pur(u)li* (house).[131] In Anatolian the loan-word ousted IE **dom-*,[132] **weik'-*. The social and/or architectural reasons for the possible place and time of the borrowing (not later than in the third millennium B.C.) deserve a separate study.

If some of the features shared by Anatolian languages appeared because of their later interference with each other and with the non-IE languages of the Near East,[133] the notion of Anatolian can be reinterpreted as a linguistic area or zone. Among the first scholars to suggest that Anatolian had been (also) a *Sprachbund,* was B. Rosenkranz,[134] one of the founders of Luwian and Anatolian studies.

The interesting feature of the area that made possible deep interprenetration of languages was the fact that they were cognate to each other. That helped features that had appeared in one dialect to spread in all the rest. Some common Anatolian features (as the demonstrative pronoun: Hit. *apa-*, Luw. *apā-*) are quite evident. But their relative chronology is not clear: if some items are found only in the Anatolian zone, it is not excluded that they might be traces of a distant (Indo-Hittite or even Nostratic) past. Thus what we call Anatolian sometimes may be really older than dialectal IE[135] (in cases where Nostratic archaisms have been preserved). In other cases a common Anatolian isogloss is not Proto-Anatolian but may point to a later spread of a certain borrowing or a feature of an areal character. For some common Anatolian words a later origin from a form of children's talk seems evident: e.g., Hit. *lala-* (tongue, gossip = evil tongue), Luw. *lalali.*[136] Some of such *Kindersprache* words are similar, but nonidentical: cf. Hit. *atta-* (father): Luw. *tati-* ; a completely different word of the same type was introduced into Palaic: *papa-* (father) (see above on Luw. *nana-*).

Several cultural terms related to the religion of all the ancient peoples of Anatolia[137] might belong to relatively late common innovations: cf., for instance, Hit. *Arma-* (moon god), *arma-* (moon), CLuw. *Arma-, arma-*, HLuw. *arma-* (moon, month), Mil. *armpa* (?), Lyc. *armma,*[138] *rmmazata* (monthly tribute, offering).

One of the most interesting cultural terms that unite Anatolian languages is the numeral "4" (Hit. *mewa-*, Luw. *mauwa*) that is different from the rest of IE dialects. It is possible to find a term related to the IE _*kʷetwor-_ (four) in Hit. *kutr-iš-* (number) as well as probably in some other Hittite nouns.[139] In that case the latter seem to reflect an earlier stage before this important cultural innovation.

The notion of Anatolian still remains as an important cover term, but while using it one should keep in mind that it may recall forms and rules of an earlier period of scholarship (*Proto-Anatolian,* from this point of view, becoming a term possibly reminiscent of nineteenth-century methodology).

The development of an Anatolian linguistic league might have been due mainly to the geographical proximity that together with social and economic ties made bilingualism (or plurilingualism) a typical feature of the sociolinguistic situation. The distribution of the Indo-European dialects by the middle of the second millennium B.C., as seen in the map, was caused by the break of the older links between Greek (and Proto-Phrygian) and the other Eastern IE dialects (Mesopotamian Aryan or Mitanni Aryan and Proto-Armenian). A possible reason for that break might have been the westward movement of the linguistic ancestors of Greeks (and Phrygians) from a hypothetical homeland of all the speakers of Eastern Indo-European.[140] That movement led to the loss of the previous links between the Armenian and Greek dialects, on the one hand, and Indo-Iranian languages, on the other, and to the change in the topology of dialectal relations.[141] Before that, Greece and the whole Aegean area might have been occupied by the speakers of Southern Anatolian,[142] whose place at that time can be defined as Western in respect to the other part of (Eastern, future Northern) Anatolian dialects. The first movements of these two main divisions of the IE original area should have been approximately synchronous with the separation of Tocharians (as seen from the isoglosses discussed above). If the latter can be identified to Kuteans, as Henning suggested,[143] their first place of residence was not far away from a hypothetical "Euphratian" dialect, as reconstructed by G. Whittaker. With all these initial movements most of the dialects were still relatively close to a hypothetical homeland. Only later migrations (through the Balkans and the Circumpontic area to what is now Eastern, Central, and Western Europe; through the Caucasus to the steppes of the Volga-Ural space and more Eastern areas; through the Iranian ["Silk"] road to Central Asia and India) made the IE dispersal as wide as it was at the beginning of the modern era.

Supplement

TABLE IA. HITTITE-LUWIAN LEXICOSTATISTIC CORRESPONDENCES IN A 100-WORD GLOTTOCHRONOLOGICAL LIST

N/translation	Hittite[144]	Luwian	Identical (+) different (-) unknown (?)
1. all	*ḫumant-*	*punata-/tanimali-*	-
2. belly	*šarḫuwant-*		?
3. big	*šalli-*	*ura-*	-
4. bird	*šuwai-š*		?
5. bite	*wak-*		?
6. black/dark	*dankui-*	*dakkui-*	+
7. blood	*ešḫar*	*ašḫar-*	+
8. bone	*ḫaštai-*	*ḫašša-*	+
9. burn	*war-*		?
10. cloud	*kammara-, alpa-*		?
11. cold	*ikuna-*		?
12. come	*uwa-*	*awi-*	+
13. die	*ak-*	*wal-*	-
14. drink	*eku-*	*u-*	+
15. dry	*tepšu-*		?
16. ear	*ištamana-*	*tummant-*	+? borrowing from similar sources? 104 below
17. earth	*tekan*	*tiyami-*	+
18. eye	*šakui-*	*tawi-*	+
19. feather	*pattar*	cf. *aruti-* (wing)[145]	?
20. fire	*paḫḫur*	*paḫur*	+
21. fish	*parḫu*		?
22. fly	*watkuwar*		?
23. foot	*pat(a)-*	*pata-*	+
24. give	*pai-*	*piya-*	+
25. green	*ulili-,* *ḫaḫl-awant-*	*ḫaḫaliyanti-*	+
26. hair	*tetana-*	*tappani-*	+? above, section 43
27. hand	*keššar*	*iššari-*	+
28. head	*ḫarša-r-/-n-*	*ḫarma-ḫi*	-
29. heart	*ker, kardi-*	*zarza*	+
30. kill	*kuen-*		?
31. know	*šak-/šek-*	*un(a)i-*	-
32. leaf	*ḫurpašta(n),* *tarša-*	HLuw. *tarsa-*	?
33. lie	*ki-*	*zi-*	+
34. liver	*lišši-*		?
35. long	*daluki-*	*aryu-*	-
36. man (adult)	*antuḫša-*	*maya-šši*	-
37. many	*mekki-*	*mayi-*	+
38. meat	*miluli-*		?

N/translation	Hittite[144]	Luwian	Identical (+) different (-) unknown (?)
39. mountain	*kalmara-*	*ariyatt-*	-
40. mouth	*aiš*	*aš-*	+
41. name	*laman*	HLuv. *a-ta*$_{4/5}$*-ma-za*	+
42. new	*newa-*	*nawa-*	+
43. night	*išpant-*		?
44. person	*twekka-*	HLuw. *atri-*, Lyc. *atra/atla-*	? (cf. Hit. *ešri-* [essense])
45. rain	*ḫeu-*		?
46. red	*mita-*		?
47. road	*palša-*	*ḫarwa-* (< Hurrian) cf. HLuw. *pal*(*a*)*sa-*?	-
48. root	*šurki-*		?
49. see	*auš-*	*mana-*, *mimma-* (cf. *awa-* [behold])	+
50. seed	*warwatn-*	*warwalan?*	+?
51. sit	*e/aš-*	HLuv. *as-*	+
52. skin	*kurša*		?
53. sleep	*še/aš-* (*tešḫa-* [dream])	*duntariyašḫa-* [dream]	-?
54. small	*kappi-*		?
55. smoke	*kammara-*		?
56. stand	*ar-*		?
57. star	*ḫašterza*		?
58. stone	*paššu*, *aku-*	HLuw. *asu*	+?
59. sun	*šittar* (?)	*Tiwat* [Sun-god]	?
60. that	*apa-*	*apa-*	+, common Anatolian
61. this	*ka-*, *ki-*	*zi-*	+
62. thou, you	*zik*	*ti*	+
63. tongue	*lala-*	*lala-*	+ (Kindersprache)
64. tree	*taru*	*taru-*	+
65. two	*da-*	HLuv. *tuwa-*	+
66. walk-	*iya-*	*i-*	+
67. warm/hot	*ant-*		?
68. water	*watar*	*war*	-
69. we	*weš/anz-*	*anz-*	+
70. what	*kuit*	*kui* (?)	+
71. white	*ḫarki-*		?
72. who	*kuiš*	*kuiš*	+

TABLE IB. HITTITE-LUWIAN LEXICOSTATISTIC CORRESPONDENCES IN THE NEXT PART OF THE 200-WORD GLOTTOCHRONOLOGICAL LIST

N/translation	Hittite	Luwian	Identical (+) different (-), unknown (?)
73. moon	*arma-*	*arma-*	+
74. full	*šuwa-nt-*	*šuwa-mma-nza*?	+?
75. knee	*genu*, *ḫaliya-* (to kneel down)	*ḫal-ḫal-zani-* (?)	+?
76. horn	*šawa/it*(*a*)*r*-, *karawar*	*zarwani*(*ya*)	+
77. breast	*taggani-*		?
78. and	*-ya*	*-ḫa*	-
79. animal	*šuppal-/huitar*	*ḫuitar* (?)(?), HLuw. *hwis/tara*	? Hit. borrowing, cf. 115
80. back	*iškiša-*		?
81. bad	*ḫuwappa-/idalu-*	*adduwal-*	+
82. because	*kuit*		?
83. blow	*parai-*		?
84. count-	*kappuwai-*		?
85. cut	*kuer-*	*kur-*	+
86. day	*šiwatt-*	*ḫaliya-*	-
87. dig	*padda-*		?
88. dirty	*iškunant-*		?
89. stupid, dull	*marlant-*		?
90. fall	*mauš-*		?
91. far	*tuwa*(*la*)-		?
92. father	*atta-*	*tati-*	+? Kindersprache
93. fear	*naḫšaratt-*	*kuwayatt-*	-
94. few (small)	*tepu*		?
95. fight	*ḫulḫuliya-*		?
96. flow	*arš-*	*arš-iya-*	+
97. flower	*alil-*		?
98. four	*mewa*	*mawa*	+
99. fruit	*šeša*	*allatar* ?	-?
100. good	*aššu*	*waššu*	-
101. grass	*zuḫri*	*zuḫrit*	(+) Hurrian borrowing
102. guts	*karat-*	cf. *zarza*, above, 29	+
103. he	*a-*	*a-*	+
104. hear	*ištam-aš*	*tumant-iya-*	(-), cf. 16 above
105. here	*ket*	*zawin*	+
106. hit	*walḫ-*		?
107. hold	*hark-*		?
108. how	*maḫḫan*	*kuwapi*	-
109. hunt-	*parḫ-*		?
110. I	*uk/amu-k*	*amu*	+
111. if	*takku/man*	*man*	+
112. in	*anda*	*anta*	+
113. laugh	*ḫaḫḫarš-*		?
114. leg	*walla/ulla*		?
115. live	*ḫueš-*	*ḫuet-*	+, cf. 79

N/translation	Hittite	Luwian	Identical (+) different (-), unknown (?)
116. mother	*anna-*	*anna/i-*	+
117. narrow	*hatku*		?
118. near, short	*maninku-*	*mannakuna/i-*	+
119. not	*natta/ le*	*nawa / niš*	-
120. old	*wezzapant-*		?
121. one	*ašma-*		?
122. other	*damai-*	*ni-waralla/i* - (alien)	?
123. pierce	*iškaratar*		?
124. play/enjoy	*du-šk-*		?
125. pull	*ḫuittiya-*		?
126. push/ throw	*peššiya-*		?
127. right (side)	*kunnaz*	*išarwi*	-
128. right	*ara*	Lyc. *ara*	+
129. rope	*išḫimana*		?
130. salt	*izzan*	*al-*	-
131. say	*mema-*		?
132. scratch	*ḫaḫḫariya-*		?
133. sea	*aruna*	*al-*	-
134. short	*maninku-*	*mannakuna/i-*	+
135. sing	*išḫamai-*		?
136. sky	*nepiš*	*tappaš*	+
137. smooth	*dannara-*		?
138. snake	*illuyanka-*		?
139. some	*kuiški*		?
140. spit	*išpatar/allapah-*	*tappa-?*	-?
141. split	*ḫappešnai-*		?
142. squeeze, force	*damaš-*		?
143. stick	*ḫattai-*		?
144. straight	*ḫarši, innaru-* (?)		?
145. suck	*unḫ-*		?
146. swell	*putkiya-*		?
147. there	*apiya*		?
148. they	*-e, -at*	*-ata*	+
149. thin	*ḫaḫḫar-*		?
150. think-	*kappuwai-*	*mal(a)i-*	-
151. this	*ka-, ki-*	*zi-*	+
152. three	*teri-*	HLuw. *tari-*	+
153. throw	*peššiya-*		?
154. tie	*išḫai-*	*ḫišḫiya-*	+
155. turn	*weḫ-*	*kuwal-/zal-*	-
156. wash	*arra-*	*ililha-*	-
157. when	*kuwapi, maḫḫan*	*aḫḫa*	-
158. wide	*tuwa(la)-*		?
159. wind	*ḫuwant-*		?
160. wing	*partawar, pattar*	*aruti,*[145] *partianza* (?)	+?
161. wipe	*anš-*		?
162. with, under	*katta*	*annan*	-
163. you	*šumeš*	*u-nza*	-
164. year	*wett-*	*uššai-*	+
165. build	*wede-*	HLuw. *tama-*	-
166. pure	*parkui-/šuppi-*	*kumma -*	-

TABLE 2. ETYMOLOGICAL CORRESPONDENCES TO THE OLD HITTITE TEXTS IN LUWIAN

N/translation	Hittite	Luwian	Indo-European	Identical (+) different (-) unknown (?)
1. sky	_nepiš-_	_tappaš-_	_*ne/obh-es-_	+
2. he	_a-_	_a-_	_*e/o-_	+
3. local/aspect particle	_-šta_	_-tta_	_*sdh_ H_2 _e_	-
4. god of Storm	_Tarḫunn-aš_	_Tarḫ-unt-_	_*te/orH-w(e/o)nt-_	+
5. good, kind	_aššu-š_	_waššu-š_	_*e/osu-s_	-
6. to be	_eš-_	_aš-_	_*es-_	+
7. introducing conjunction	_na-_	_pa-_	_*n(o)-_	-
8. if, when	_man_	_man_	_*m(V)-n-_	+
9. king	_ḫaššu-š_	_ḫant-a-wat-i-_	_*hons-u-us_	-
10. under, with	_katta_	_annan_	_*km-t-o_	-
11. with much power	_pangar-it_	_muttiyanza_	_*bhn̥g̑h-r-_	-
12. night	_išpant-i_	cf. Cappadocian _išpatalu_[146]	_*(we)kse/op-(e/o)r-/-(e/o)nt-_	+?
13. to take	_da-_	HLuw. _ta-_	_*doH_$_2$_-_	+?
14. bad, evil	_idalu-š_	_adduwal-_	_*ed-w-(e/o)l-_	+
15. not	_natta_	_nawa, na?_	_*ne-t-_	+
16. anybody	_kuiški-_	cf. _kwišḫa_ (some-one)	_*kwis-(k_^{w}_e)_	+, cf. 24
17. to make	_takkeš-_	cf. _takkiš(ša)rai-_ (ornament)?	_*tek-s-_	+?
18. mother	_anna-_	_anna/i-_	_*anna-_ (< child's word)	+
19. father	_atta_	_tati-_	_*atta-_ (< child's word)	+
20. to do	_iya-_	_aya-_	_*Vye-_	+
21. back, after	_appan_	_appan_	_*(e/o)p-om_	+
22. first, same	_šaniya, hantili-_	-?	_*som/n-_	-? cf. 92
23. year	_witt-i_	_ušša/i-_	_*u/w(e)t-(so)-_	+
24. which	_kui-t_	_kwit_	_*kwi-t/d_	+, cf. 15
25. emph. particle	_pat_	-	_*pot_	-
26. raise, check	_arai-_	_ari(ya)-_	_*e/or-_	+
27. all	_ḫumant-_	_punat-, tanima/i-_	_*H(e/o)m-n-_	-
28. warmth	_ḫantaiš-_	-?	_*HV-nt-_	-?, cf. 87
29. time	_meḫur/n-_	_ara/i-_	* _meH_$_1$_-ur/n-_	-
30. consecrate	_ḫapp-ar-iya-_	cf. _ḫapp-in-atta-_ (wealth?)	_*Hop-_	+?
31. me	_amm-el_	cf. HLuw. _am-i-_	_*(e)m-_	+, cf. 57
32. to become	_kiša-_	-	_*g'hes-?_	-?

N/translation	Hittite	Luwian	Indo-European	Identical (+) different (-) unknown (?)
33. and	_-(y)a_	_-ḫa_	*_yo_	-
34. to settle, to populate	_aš-as-_	cf. HLuw. _is-anu-_	*_ēs-ēs-?_	+?
35. this	_apa-š_	_apa-š_	*_e/obh-e/o-_	+
36. locative/ aspectual particle	_šan_	_-tta_	*_som_	-
37. sea	_aruna-_	_al-i-_	*_Vr-wen-_	-
38. that	_ki-_	_zi-_	*_k'i-_	+
39. word	_uttar_	_utar_	*_uk_^{w}_-t(o)r_	+
40. prohibitive negation	_le_	_niš_	*_le?_	-
41. second time	_dan_	cf. HLuw. _tu-wa/i-zi?_	*_d(w)o(yo)n?_	+?
42. to bring	_uwate-_	_uwata-_	*_aw-oH_$_1$ _-dheH_$_1$_?_	+
43. and	_š(u)-_	-	*_s(o)-_	-
44. formerly	_karu_	_puwa_	*_g=k'(V)r-u-_	-
45. carry off	_peda-_	_padd-?_	*_pe- dH_$_3$.	+?
46. (Sun) god	_Šiu-_	_Tiu-at-_	*_dei-w-(ot-)_	+, but cf. 64
47. our (possess.)	_-šummi-_	cf. HLuw _-a-zi_	*_s-uw-?_	-?, cf. 95
48. living, alive	_ḫuis-want-_	_ḫuit-wa-_	*_Hwed/s-?_	+
49. starve	_kišt-_ (_kašt-_ [hunger])	_ki/ašt-?_ HLuw. _asati_ (hunger)	*_ghe/os-t-_	+?
50. forth	_para_	_pari_	*_per-o-(m)_, different cases of a relational noun	+
51. to go	_pai-_	_i-_, HLuw. _pa-?_	*_pe/o-_ + *_ei-_	+, cf. 66, 69
52. place	_peda-_	HLuw. _pita-_	*_ped-o-m_	+
53. his/her/its	_š-i-_	-	*_s-(ei)_	-
54. carry out	_aniya-_	_anni-/an(i)ya-_	*_enH-yo-_	+
55. strike	_ḫazziya-_	HLuw. _ḫa-zi-_	*_Hot-_	+[147]
56. face	_mena/i-_	_mannaḫ-?_	*_men-_	+?, cf. 59
57. my	_me-_	_-mi_	*_m-(e,-i)_	+, cf. 31
58. to turn to	_nai-/ne-_ (also redupl.)	_nana-_ (redupl.)	*_neH-_	+
59. in face of	_mena-ḫḫanda_	_ḫant-_	*_men-o-h(V)nt-_	+, cf. 56
60. pole of a carriage, (wooden) weapon	_turi-_	_turi-_	*_dhur-_	+(IA ?)
61. to lead (army)	_huittiya-_	_-wat-_	*_Hwet-_	-? (loss of *_H_ in Luw?)
62. to pray while sacrificing	_malt-_	-	*_mel-dh-_	-?
63. to extract, to conjure	_ḫuwart-_	_ḫuwart-?_	*_Hwer-dh-?_	+?
64. day	_šiwatt-_	- (substituted by a borrowing)	*_diew-ot-_	-, but cf. 46
65. in(to)	_anda_	_anda_	*_en-d-o_	+(IA?)
66. to come	_uwa-_	_awi-_	*_aw-_ + _ei-_	+, cf. 51,
67. to set fire, to light	_lukk-i-_	_-?_	*_leuk-_	-?
68. encircling	_ḫulal-eššar_	_ḫula-_	*_Hwel-_	+

N/translation	Hittite	Luwian	Indo-European	Identical (+) different (-) unknown (?)
69. to march	*iyannai-*	*i-*	**ei-*	+?, cf. 51, 66
70. fight, battle	*lahha*	*lahhiya-*, *lalhiya-*	**le/oH-*	+
71. present	*heng-ur*	*-?*	**He/onk/g-*	-?
72. on the right	*kunn-az*	*išarwi*	**kwen-?*	-
73. to beget	*ḫaš-*	cf. *ḫamš-* (grandchild)	**He/om/ns-*	+
74. dung	*š/zakkar*	-	**skor*	-?
75. to fill	*šunna-*	*šuna-*	**sew-n-*	+
76. to put in an intensive way (many objects)	*zikk-*	-	**dH-sk'e/o-*	-
77. to leave	*tar-na-*	-?	**te/or-k-/-n-*	-?
78. up	*šara*	*šarra*, *šarri*	**se/or-*	+
79. take	*da-*	*ta-mma-* (taken, captured; pass. part.?)	**doH-*	+
80. great	*šall-i-*	*šalḫ-?*	**solH-*	+?[148]
81. between	*ištarna*	-	**ste/or-n-o*	-?
82. further, then	*namma*	-	**no-mo*	-?
83. reach	*ar-*	*-?*	**Vr-*	-?
84. say, talk	*tar-*	*tatar-* (curse) (redupl.)	**te/or-*	+
85. here . . . (in the very beginning of direct speech)	*ka-ni*	-	**k-*	-
86. (direct speech [particle])	*wa(r)-*	*wa-*	**wer-*	+
87. to warm	*i-nu-*	-	**(H)ai-*	-, cf. 28
88. to copulate, testicles	*ark-*	*-?*	**org'h-*	-?
89. where (. . . to)	*kuwapi*	*kuwar(i)*	*k^{w}*o-*	+
90. we	*weš*	*anza-*	**wes*	-
91. reflexive particle	*za*	*ti*	**ti*	+
92. for the first time, once	*ašma*	-?	**sem-*	-?, cf. 22
93. to disappear	*mer-*	-	**mer-*	-
94. heart	*kart-*	*zarza*, *zart-*	**k'er(d)-*	+
95. their	*šmi-*	-	**s-m-/-w-?*	- cf. 47
96. speak	*mema-*	*mal-*	**(me)mon-*	+
97. to strive	*šanḫ-*	*šanni-* (to overturn?)	**se/onH-*	+?
98. to find	*wemiya-*	cf. HLuw. *wami-LITUUS?*	**wem-*	+?
99. other	*tamai-*	-	**dom-?*	-?
100. sister	*neka-*	*nana-sr-i-* (redupl. in the first stem)	**neg'h-o-*	+
101. legal, according to the law	*ara*	*ara*	**ar-o-*	+ (IA borrowing?)

TABLE 3. ETYMOLOGICAL CORRESPONDENCES TO THE LUWIAN TEXTS IN HITTITE

N\translation	Luwian	Hittite	Indo-European	Identical (+) or different (-)
1. evil	*aduwal-i-*	*idalu-*	**ed-w-al-*	+
2. hand	*iššar-i-*	*keššar*	**gh'es--r̥*	+
3. adult	*maya-(šši)-*	- (= *pangawaš*)	**meg'h-?*	-
4. new	*nawali-*[149]	*newa-*	**newo-*	+
5. locative enclitic	*-ta*	- (functionally = *-kan*)[150]	**-t*	- (Hittite)
6. head	*ḫarma-ḫi*	- (*ḫaršar*)	**HVrmo-*	-
7. (over)power	*muwa-*	- (*tarḫ-*)	**meug-?*	-
8. eyes	*tawa*	*šakuwa*	**s/tok*^{w}*-o*	+
9. part of the body-scheme	*ḫappiš-*	*ḫapeššar*[151]	**Hop-*	+? (IA?)
10. this	*zi-, za-*	*ki-*	**k'i-*	+
11. body	*waššinali-*	-(*twekk-*)	**m/wes-?*	-
12. left	*ipala-*	?(GÙB-*la-*)	**g'hebh-l-*	?
13. make run	*ḫui-nu-*	*ḫuile-nu-*	**Hwei-n(e)u-*	+
14. right	*išarui*	-(*kunna*)	*(*is?*)-*Vr-w-*	-
15. recite(?) charms(?)/take	*lala-* (redupl.)	*?da-*	?**doH-*	+?,[152] cf. 66
16. chase, announce(?)	*papra-* (redupl.), *para-*	cf. *parḫ-?*	**pelorH-?*	+?
17. curse	*ta-tar-iya-man*	*tar-*	**telor-*	+
18. oath, curse	*hirut-*	-	**H_2erud-*[153]	-
19. God of Thunder	*Tarḫ-unt-s* = *Tarhunza*	*Tarḫuwant-*	**torh-u-nt-*	+? (IA?)
20. *to regard	*mammana-*	-	**men-*	-[154]
21. that	*apa-*	*apa-*	**VbhV-*	+ (IA)
22. life	*ḫuit-wala-ḫit-*	*ḫueš-*	**Hwes/t-*[155]	+
23. forceful, virile	*annar-umm(i)-*	*innarawant-*	*(*H*)*Vnelor-*	+
24. long	*array(a)-*	- (*daluki-*)	**Vr-eloi-*	-
25. time	*arai-*	- (*mehur*)	**Vr-*	-
26. health	*ḫatt-ula-ḫit-*	**ḫatt-ul-*[156]	**Hot-*	+?
27. favor	*wašš-ar(-ahit)-*	- (cf. *ašš-u*)	**welos-*	-
28. raise	*ari(ya)-*	*ara(i)-*	**Vr-*	+
29. to cut	*ku(wa)r-*	*ku(e)r-*	**k*^{w}*er-*	+
30. to cut	*karš-*	*karš-*	**ker-s-*	+
31. mobile wealth	*iuna-ḫit* -	cf. *i-tar* (road = going)	**ei-*	+, cf. 85
32. if, when	*man*	*man*	**m-*	+
33. reflexive particle	*-ti*	*-za*	**t-*	+
34. introducing enclitic element	*-pa*	-	**-p(o)*	-
35. enclitic pronoun, 3 p.sg.	*d-u*	cf. *ta-* (and)[157]	**-t-?*	+

N\translation	Luwian	Hittite	Indo-European	Identical (+) or different (-)
36. strike	_dupa-_	-(_walh-_)	*_dheubh-_[158]	-
37. prohibitive negation	_niš_	-(_le_)	*_nei-_	-
38. demon. pr. 3 p.p.sg.	_a-_	_a-_	*_e/o-_	+
39. wash (off)	_e/il-e/ilḫa-_	_laḫuwai-_ (pour)	*_leHw-_ (pour)	+
40. to be	_aš-_	_e/aš-_	*_es-_	+
41. god	_maššana-_	-(_šiuna-_)	*_mes-_	-
42. before	_parran_	_peran_	*_per-o-m_	+
43. four	_mauwa_	_mewa-_	*_mei-w-?_	+ (IA)
44. part of the body(?), leg?	_part-_	-	*_pVrt-_	-?
45. sinful	_waškuwwalli-mali-_	_wašt_(a)- (sin)	*_wVs-t-_	+
46. now	_na-nun_	_ki-nun_[159]	*_n_(_o_)- + _nun_	+
47. year	_ušša-_	_wet-_	*_wet-_	+
48. back, again	_appa_	_appa_	*_VpV_	+
49. which	_kuiš_	_kuiš_	_kui-s_	+
50. to curse	_tiw-at-an-iya-_[160]	_šiw-att-_	*_deiw-_	+
51. wine, mead	_maddu-_	_wiyana-_	*_medhu_	-
52. honey	_mallit-_	_melit-_	*_mel-i-t-_	+
53. to flow	_aršiya-_	_arš-_	*(_e/o_)_r-s-_	+
54. to make	_aya-_	_iya-_	*(_V_)_ya-_	+
55. to die	_uwal-ant-_	- (_akk-_)	*_wVl-_	-
56. brother	_naniy-a_ (of a brother), HLuw. _atala-?_	_neg-na-_	*_neg'h-_	+
57. sister	_nan-ašr-i-_	_-šara-_	*(_e/o_)_sor-_	+, cf. 15
58. division(?)	_kuwarša-_	-	*k^w_Vr-_[161]	-
59. sky	_tappaš-_	_nepiš-_	*_ne/obh-es-_	+
60. earth	_tiyamm-_	_tekan_	*_dheg'hom-_	+
61. all	_punata-_	- cf. _panku-_ (all, total)	*_pṇ_k^w_-t-_	-
62. you (2 p.pl.)	_unza_	- (_šumeš_)	*_u-_	-
63. and	_-ḫa_	- (-[_y_]_a_)	*_-He/o_	-
64. enclitic particle	_-tar_	-	*_-tṛ_	-
65. carry out	_anniya-_	_an_(_n_)_iya-_	*_en-yo-_	+
66. as	_aḫḫa_	- (cf. _m-aḫḫa-n?_)	*_VHV_	-
67. reed	_nata-_	_nata-_	*_nodo-_	+
68. break	_tatarḫ-_	_tarḫ-_ (to overcome)	*_te/orH-_	+
69. strike	_wida-_	-	*_wedh-_ (?)	-
70. under	_annan_	- (_katta_)	*_Vn-_	-
71. foot	_pata-_	_pata-_	*_pe/od-_	+
72. put	_tuwa-_	_dai-_	*_dhV_H_2-	+, cf. 15
73. nails	_tammuga_	_šankui-š_	*_t/som_g^w_h-_	+
74. peg, nail	_tarm-_	_tarm-_	*_ter-m-_	+
75. up	_šarra_	_šara_	*_se/or-_	+
76. not	_na-wa_	_na-tta_	*_ne_	+
77. great	_ura-_	- (_šall-i-_)	*_wer-_	-
78. father	_tati-_	- (cf. _atta-_)	*_ta-ta_ (affective _Kindersprache_)	-

N\translation	Luwian	Hittite	Indo-European	Identical (+) or different (-)
79. grandchild	_ḫamša-_	_ḫanza_	_*Homs-_	+
80. pound	_puwa-_	_puwa-_	_*pVw-_	+? (IA?)
81. grandfather	_ḫuḫa-_	_ḫuḫḫa-_	_*HauH-o-_	+
82. mother	_anni-_	_anna-_	_*anna-_	+
83. to feed	_adari-_	_etriya-_	_*ed-r-i-yo-_	+
84. bone	_ḫaš-_	_ḫaštai-_	_*Hos-(t)-_	+
85. come	_aw-i-_	_uwa-_	_*Vw-_ + _*ei-_	+, cf. 26
86. blood	_ašḫar-_	_ešḫar-_	_*esHor_	+
87. heart	_zarza_	_kir, kart-_	_*k'elord-_	+
88. past	_puwa-til /-la_	-	_*bhuH-_	-
89. ear	_tumant-_	_ištamana-_	_*(s)tm-n_ (?)	+?[162]
90. sheep	_ḫawi-_	? (_UDU-iyant-_)	_*Howi-_	+?
91. horn	_zarwani-_	_karawar/-n-_	_*k'r̥welor_	+
92. frightfulness	_ḫatta-štara/i-_	_ḫatta-_	_*Hot-?_	+
93. wood > statue	_daru-ša_	_taru_	_*doru_	+
94. high	_parray(a)_	_park-u-_	_*bhr̥g'h-(i)_	+
95. rapid	_zariya-_	-	_*ske/or-_	-
96. water	_wid-(anza)_	_wid-/wat-ar/n-_	_*we/od-_	+
97. give	_piya-/pai-_	_pai-_	_pe/o-_ + _ai-_	+
98. word, spell(?)	_utar/n-_	_uttar_	_*u(e)k-r(o)r?_	+?
99. *binding	_ḫišḫi-šša-_	_išḫiya-_ (to bind)	_*(V)sH-yo-_	+
100. distrain	_ip(pa)tarri(ya)-_	_appatriye-_	_*_(H_1)_Vp(V)tr-yo-_	+
101. hand over	_darawi(ya)-_	-	_*doH-r-u-_ (gift?)	-, cf. 91
102. sevenfold	_šap(pa) ta-mmimma/i-_	_šiptamiya -_	_*septem-yo-_	+
103. slanderous	_mal/ntalla/i-_	_mandalliy-eš_	_*mend-?_	+? (IA?)
104. rock-face	_wani(ya)_	-	_?*_g^w_Vr/n-_	-
105. eat	_ad-_	_e/ad-_	_*ed-_	+
106. know	_uni-_	_ša/ek-_	_*gnoH-_	-

Abbreviations for notes and references

2 Bo TU = Forrer Emil. Boghazköi-Texte in Umschrift. II. Leipzig, 1926
KBo = Keilschrifttexte aus Boghazköi. Leipzig/Berlin 1916-
Kl Spr = Friedrich Johannes. Kleinasiatische Sprachdenkmäler. Berlin
KUB = Keilschrifturkunde aus Boghazköi. Berlin, 1921–
St Bo T = Studien zu den Boğazköy-Texten. Hrsgb. Von den Komission für den Alten Orient der Akademie der Wissenschaften und der Literatur. Wisbaden: Otto Harrassowitz, 1965-

Notes

1. From these two possible conventional designations one would select the first mainly because at the time of the composition of the Old Hittite (OHit.) laws, Luwia (and later Arzawa) as a southern country was opposed to the central and northern area (where the OHit. language was spoken). The term corresponds also to the geographical situation of Northern Anatolian (NA) Palaic (Pal.). NA Lydian (Lyd.) (that in some respects is closer to SA) was spoken (at least at the classical time when the texts in it were composed) in a relatively more northern part of the central area of the western Anatolia. During the same classical period Lycian (Lyc.) and more archaic but less understandable Milyan (Mil., "Lyc. B"), continuing a Luwian dialect, and most of the lesser known SA dialects that have been discovered and studied recently (Pisidian, Carian, Sidetic) were spoken in the SW Anatolia. The linguistic situation of Troy is still not absolutely clear. It seems possible that the Trojan military alliance could have included participation by Luwians (Watkins 1995, with references), Thracians (linguistically comparable to Balto-Slavic), and Phrygians belonging to the Eastern Indo-European (Indo-Iranian-Greek-Armenian) dialectal group (see Gindin 1999 on Luwian, Thracian, and Phrygian as constituents of the Trojan population and their interrelationship), as well as some other "peoples of the sea" and (Proto) Etruscans. Thus, in Troy and its war one may see both an early example of a social unit in which members of different IE dialectal groups were integrated and also a first-known conflict between speakers of separate IE groups including Greek (Gk.). In the interval between the end of the Hittite Empire and the Trojan War, on the one hand, and composition of Lycian A texts, on the other hand, the only SA dialect known through written documents was Hieroglyphic Luwian, spread in southeastern Anatolia and in northern Syria to the southeast (see a map supplemented at the end of the vol. 3 of Hawkins 2000).

2. Morpurgo-Davies, Hawkins 1986; Melchert 1987. As Luw. preserved some labiovelars (particularly voiceless and voiced aspirated), SA may be compared to those satem dialects that (as Armenian and Albanian) have shown traces of all three distinct rows of "guttural" consonants: velars, labiovelars, palatals. Possible traces of old labiovelars are supposed in some positions also in Sanskrit and Balto-Slavic. Thus existence of a labiovelar row is not relevant for the initial satem change. But (as in the other dialects of this type) in Luw. labiovelars tended to disappear mainly due to their secondary palatalization (Ivanov 1999a). Although in Luw. the palatal voiced aspirate _*g'h_ disappeared without developing into a fricative (but an intermediary stage _*y=j_ was possible; cf. a change like Skt. _h<*g'h_), the main phonological feature that united all these processes may be seen: the ancient row of velars had been kept distinct from the old palatal row while the labiovelar one shifted slowly to the other parts of the consonant system.

3. Szemerényi1970; Morpurgo-Davies, Hawkins 1986.

4. Nussbaum 1986, 31 and o. (see index, s.v.); Melchert 1994, 234 a.o. (see index, s.v.). Bonfante and Gelb (1944) had already discovered the satem character of HLuw. on the basis of this word and two others with the same development of *k'>s (see on the reading of HLuw. sign as sù: Hawkins 2000, 35–36).

5. Melchert 1994, 94 (with references); Hawkins 2000, 153, 444.

6. Ivanov 1999d, 193–94 n.294 ff. The non-double spelling for an IE voiceless stop in this position occurs also in Hit. *aku-* (stone), HLuw. *asu-* (Hawkins 2000, 127, 155).

7. Ivanov 1999a, 36–37; 1999d, 185–88.

8. The reconstruction *s_{mobile} may designate an IE segmentary unit different from **s-* in other positions, cf. a hypothesis in Gamkrelidze, Ivanov 1995, 1:104. Inside a word there are definite cases with a correspondence Hit. *-š-*: SA *-t-* like Hit. *ešri-* (shape, form, image, essence): HLuw. *atri-* (form, figure, image, soul) (Hawkins 2000, 460), Lyc. *atr/la-* (self, person) (Melchert 1994, 272–73 with discussion of possible explanations).

9. Melchert 1993a, 71. OHit. *šakuwa* seems to preserve traces of a collective dual form (of Nom.-Acc.-Dat.) as, for instance in a construction with a postpositional enclitic pronoun *ša-ku-wa-aš-ma* (2 BoTU 12 A= KBo III 34 I 18).

10. This etymology seems preferable to the traditional comparison to Lat. *sequor* (cf. Orel 2000, 184) for semantic and formal reasons. The difference between the initial phonemes in Albanian words for *eye* and *to see* is explained by the early palatalization that distorted the first of them, leading to the loss of the original etymological connection (in Germanic a similar result was due to Verner's law).

11. See on the Lith. suffix: Otrębski 1965, 265, §441 (without the OP parallel). Since in 2 dialects of the satem type (Armenian and Luwian) a sequence *-ug-* is similar to the corresponding centum Gk. *-υχ*, it is possible to reconstruct such dialectal variants as **ugh/-gh*^{w}-. See references on non-Anatolian dialects: Lehmann 1986, 145–46, G 48; Van Windekens 1976, 277.

12. The connection to a word for *water* suggested in several etymological dictionaries may refer to a later popular etymology. See Melchert 1994, 258 on the relation between the Hit. and Luw. words.

13. Melchert 1994, 229, 263.

14. It seems that to reconstruct a protoform for this Hit. word that has been considered as a crux (Melchert 1994, 115–16), one has to use Nostratic/"Eurasian" comparisons: Altaic **ak'V-* (mouth; gill) > Proto-Turkic **agy-r'*, Proto-Korean *aku-*, Proto-Japanese *aki-* (Starostin 1991, 283 n.189; 167 n. 56; 256 n.148; 271 n.163; see also on a synonymous, but probably not cognate, Nostratic **Haŋa* [to open a mouth] > Uralic, Altaic **aŋa* [Starostin, 46–47, 81*]), Eskimo-Aleut **a(g?)i(-)ttaR-* (to open one's mouth), [cf. Fortescue, Jakobson, Kaplan 1994, 8], Aleut *agi-x'ta* (to yawn), *agi-lgi-x'* (*gate <* mouth*), Bergsland 1994, 23. The closest parallel to a form continued in Hittite might be found in Old Turkic *aɣiz*. Hittite has preserved a similar word structure with a loss of the intervocalic *-H-* <-***G-*: Hit. *ai-š-/ i-šš-* with an archaic IE quantative ablaut, no trace of which is seen either in Luw. or in the other IE dialects Ivanov, 2000, 7–8. A supposed Pal. cognate is unclear. In some IE there is *-u-* developed after the loss of a laryngeal.

15. This vowel may be unaccentuated in Hit. as Melchert 1994, 135, suggests, but there are no semantic and comparative reasons for the reconstruction of an initial morph **en-* in this word; see a stimulating anlysis in Puhvel 1984, 372–73.

16. A hypothesis on an IE character of the form (cf. Oettinger 1979, 107–8, 328, 483)

can be supported by a probable Nostratic (Kartvelian) parallel: Svanetian (*li-)d-esg-*, see Ivanov 1981, 13–14, 219. A Hit. reflex of a stem containing a laryngeal in the second syllable in Hit. *daluki-* (long) is not clear: Melchert 1994, 67, and see on *daluki-* below, footnote 97.

17. The suggestion of a secondary development of a lost phoneme (Melchert 1994, 69, 321) does not seem plausible. But if CLuw. *duttariyati-* is really connected to the word *daughter* (Melchert 1993a, 238, following Starke), the probable solution for this word might be a borrowing from Hittite: Hit. *duttar*(*i*)*yat*(*iy*)*aš* shows the loss of a laryngeal/schwa normal for that language and comparable to forms like Oscan *futir*, ON *dōttir* with a simplification of the intervocalic group of consonants after the loss of a schwa, cf. Goth. *dauhtar* (Eng. *daughter*), Lith. *duktė*, OP *duckti*, OCS *dŭštĭ*, Arm. *dustr*, Av. *duγδar*.

18. In Hit. *waš-* (to buy) (: Skt. *vas-na-*), HLuw. *iyasa-* (Hawkins 2000, 418), and Lyc. *ije-* (to buy), the distribution of the initial *-w-*/*-0-* between NA and SA seems to be just opposite to this pair of words.

19. Recently it was supposed that this form is significant for dialectological classification of Tocharian. As far as Hittite is concerned, one might speak of a shared archaism in respect to Toch. A while a loss of the first consonant in Toch. B *keṃ* shows a beginning of the process similar to that in a dialect that evolved into Balto-Slavic.

20. On different possible explanations of HLuw. *takam-* (*ta-ka-mi-i* [for the land]) (Hawkins 2000, 467) similar to the Hit. one (a NA borrowing?), see Melchert 1994, 254; Ivanov 1999a, 39. Probably the palatal consonant was depalatalized before a sonorant in a stem **dhelogʹm->tkam-* (cf. Toch. A *tkaṃ* = Hit. *tgan*) as in some other cases in several intermediary satem dialects (see Hamp's view of Albanian).

21. The original **-y-*, lost in most cases in Baltic, is reconstructed on the base of the gen. pl. (Lith. *žemių* Endzelīns 1971,88, 97; Stang 1976, 3,101, 115, 180. As the type has changed in later prehistoric Baltic, it can be assigned to the earliest Proto-B-S period. Thus a real dialectal link of SA to B-S (and not simply a convergent shift in a similar direction) seems possible for the reasons of relative chronology. It is interesting that these dialects belong to the satem group.

22. An isolated occurence together with *tappaš-ant-iš* (sky = ether) in KUB XXXV 39 II 14–15. The sense of the sentence (Meriggi 1957a, 223, cf. on the previous part Meriggi 1957b, 57–62) contradicts the definition of these Luw. *-nt-* form as ergatives (Melchert 1993a, 208, 230) based only on formal similarity to Hittite *nepiš-ant-*. On the form see Meriggi 1980, 308, II § 106.

23. Ivanov 1998a.

24. Laroche 1959, 97, 159. A later analysis seems purely formal (Kimball 1983, 437 n.20, Melchert 1993a, 231; 1994, 236, 242, 252, 254, 259, 263), and constitutes a step backwards from Laroche's intuitive breakthrough. A locative with a zero ending at the early period, to which Kimball and Melchert trace back the Luwian word, is an absolute form of the stem and thus does not explain the origin of the Luwian stem that is not a locative.

25. KUB XXXV 145 Rs. III 8'; Starke 1985, 231. See on the IE etymology (ON *dokkr*, German *dunkel*) Gusmani 1968, 92. On HLuw. *TERRA.DEUS.DOMINA* (lit. earth's divine Lady) cf. Hawkins 2000, 184. The combination *Dark (Black) Earth* has IE correspondences, for instance, in Latv. *dainas* (folk songs), in which the adjective *melna* (black) refers to *zeme* (earth) that can be seen as cognate to the Luw. word; cf. a similar semantic combination γᾶν μέλαιναν (dark earth) in Sappho (Snell 1981, 27, N 27, line 2) where the Gk. adjective is etymologically the same as that used in a *daina*. The same semantic combination of different

roots is met in non-IE (N Caucasian) Hur. *timerre ešeni* (= Hit. *tankuwai takni* in a bilingual text, Neu 1996, 247 with a discussion of previous works on this formula), Hattic *išta-razzil* (the black [dark] earth), which is equivalent (with a reverse word order) to Modern Adygh (Northwest Caucasian) *ś́cy f'yts'eri* (earth-black), Ivanov 1985, 37. The earth was considered a divine person (cf. chtonic deities—mothers goddesses of ancient Anatolia, also typologically similar *Mat' Syra Zemlja* [Wet Mother-Earth] in Russian folklore, with a possible explanation of the adjective as going back to Iranian *Sūra* in Iranian *ərədvī* Sūrā *Anahittā,* etc.). It seems possible to see in this mythopoetic formula a common feature of Mediterranean cultures and their neighbors. (From the huge literature it suffices to cite a posthumous synthetic work of a great historian: Braudel 1998, 136–37, 153).

26. Pedersen 1938, 35ff.; Gusmani 1968, 92; Kimball 1983, 495ff.; Oettinger 1987; Starke 1990, 59ff.; Melchert 1994, 102, 240; 2000b (a cautious survey of data).

27. Ernout, Meillet 1994, 471 (with references). See Hawkins 2000, 107, on HLuw. *has-ati* (by force) from *has-* (bone). Melchert (1994, 243) ascribes the simplification of a group **-st-* to a Proto-Anatolian period. But in this case one may accept either a parallel (convergent) development in Proto-Latin or a dialectal IE difference between a form without -**t(H)* or with it (a solution suggested by Benveniste before the Luw. form was found).

28. Čop 1989, 39, 53, who had proposed similar derivation, later changed his mind. In his last works on Indo-Uralic he thought about a possibility of explaining Luw. *š* by a special phonetic law that rests unproven.

29. In Indo-Iranian according to a general archaic law, in a compound the suffix is obligatorily changed to *-i-*:, cf. the type of *-i* adjective in a Hit. compound *pattar-palḫ-i-*, name of a bird (lit. wing/feather + broad); In Luw. the latter type of suffix is represented outside of a compound.

30. Whittaker 1998, 133–43.

31. Puhvel 1982. Cf. Gusmani 1968, 91–119; Erhart 1995.

32. Rosenkranz 1978, 119.

33. The suffix was fused with the root as may be seen from the form of reduplication (see section 20).

34. Despite morphological (but not semantic) objections of Melchert (1993a, 279) the meaning seems definite from the context; see on etymology, Ivanov 1999f.

35. It is written—probably due to the influence of HLuw.—with an unusual determinative $^{\text{KÁ}}$ (gate) (cf. HLuw. *PORTA*, logoram 237–38 according to Hawkins 2000, 27, 86, 255, 295). A puzzle: KÁ = Hit. *aš-ka-?*, cf. KÁ.GAL (gate of the city = *porta hepatis*). In the Hit. text of a ritual in which the Luw. form occurs, the logogram KÁ is used just before the Luw. part starts (KUB IX Vs.II 20). It is possible that the Luw. compound is its Luw. equivalent.

36. The Toch. type has been investigated by E. Lewy (1961, 84) who (following Schulze) suggested Ossetic, North Caucasian and Uralic parallels in which the second part of such a compound is a name of the *mouth*, cf. semantically the first part of the Luw. compound. On semantic parallels for *mouth* = *gate* cf. the type of Lat. *ostium* (entrance = Latv. *uosta* [port]) (Ernout, Meillet 1994, 471); cf. also above, fn. 14.

37. The suffix has been considered a characteristic derivational feature of Northwest IE, but it is also typical of Toch.

38. Mittelberger 1960. HLuv. is in this respect (as in some lexical details) closer to Hit. than might be explained by sociolinguistic factors. For contemporary Luw. nominal inflexion see Melchert 2000.

39. See on these forms and their different possible explanations Ivanov 1996, 714–15; Blažek 1999, p.165. Cf. Neu 1979, 193–96, on the history of the OHit. forms where there are no visible traces of comparable endings in *-*s-i-/-u-*.

40. Neu 1996, 104–5, n.18.

41. According to Melchert ergative in *-ant-* is a first defining morphological isogloss of Proto-Anatolian, and the split ergative is its first syntactic defining isogloss (Melchert 1994, 7). All the items connected to *-(a)nt-* in Luw. seem problematic: there is no evidence for such an abl. ending in Luw. to which the quasi-ergative has been (probably wrongly) traced in Hit.; the function of *-nt-* in Luw. might be different from the quasi-ergative; the relationship of Luw. *-nt-* and *-ša* needs to be investigated.

42. See on the present state of research: Gamkrelidze, Ivanov 1995, 233–95.

43. One of the alternative explanations of the Luw. *-ša* form might have been a borrowing from Hurro-Urartean. But for a case ending such borrowing is a relatively rare event.

44. Melchert 1993a, 32. The suffix *-u-* is repeated in reduplication; thus, it seems it had been already fused with the root (different from Hit. *-u-* in adjectives, above, 12). The use of a mediopassive participle *aruruwa-mmi-* with the designation of *the earth* is similar to the command "make roads higher" (*barganula*) in an archaic Hit. ritual. Cf. also HLuw. *á-ru-wa/i-ra/i-tu*, 3 p.sg. imperat.: Hawkins 2000, 324.

45. Illich-Svitych 1971, 266. See Hawkins 2000, 114, 185, 548, 628 on HLuw. *pa-*.

46. See on these endings Ivanov 1981a, 48.

47. Melchert (1994, 52) in his discussion of these verbal endings gives a geographical description of Eastern (= Northern) and Western (= Southern) Anatolian, which is quite close to the one suggested in the present work. The position of Lydian in this respect (as also in some other ones) is specific, probably intermediary.

48. Lyc. and Mil. forms probably connected to this stem still are not clear, Melchert 1993a, 58 and 123 (Mil. *pu-tu*, 3 p.sg. imper.). In inscriptions like (Kl Spr) 87 4 Lyc. *epñ pudē* (3 p.sg. past) was explained as *he admitted to* (Thomsen 1922, 436–37; Houwink ten Cate 1965, 95). It seems that HLuw. *pu-* in the form of 3 p.sg. imper. *pu-tu* (Karaburun §13, Hawkins 2000, 481 and 482–83) identical to the Mil. one may have the meaning *to be* ("he should not be superior . . .").

49. KUB XXXV 103+ 103 Rs. III 6–7/ KBo VIII 130 Rs. III 4'–5').

50. KUB XXXV 54 Rs. III 3.

51. KUB 43 Vs. II 4, Rs. III 30"–31". On *puwala* (*tempo passato*) see, already, Meriggi 1957b, 54 n.1. An attempt to see in the form a wrong spelling of *puwatil* (Melchert 2000a, 174 n.2) was caused mainly by the lack of information on the structure and meaning of the word that morphologically is similar to *zi-la* (in the future, following).

52. For analysis of the forms in different IE dialect, see Stang 1942, 198; 1966, 380; Toporov 1975, 210; Ivanov 1981a, 177–83, 197–98; Jasanoff 1997; Schirmer in Rix 1998, 93–95; Katz 2000, 9,11. The Luw. form (the non-reduplicated character of which is particularly exceptional as reduplicated verbal stem of the perfect-like type widely represented in Luw.) confirms the Jasanoff's conclusion about the non-reduplicated form of **bhuH-* in IE. But it is possible that in Luw. *puwa* a perfect/past form has been preserved that is more archaic than those analyzed by Jasanoff.

53. See Melchert 1994, 180, on a possible interpretation.

54. Wackernagel 1924, 87; Ivanov 1981, 44.

55. = Phoenician *l-pnm*, cf. Hebrew *lĕpānīm* (before, previously), Karatepe, XXXIII, Hawkins 2000, 53.

56. Meriggi 1980, 253–54, §8; 347, §219; Gusmani 1986, 131; Ivanov 1981a, 147.

57. On the history of these forms see Ivanov 1981b; 1999d, 238 with references. On the relevance for Indo-European dialectology cf. Rosenkranz 1978, 134–35.

58. Despite a structural similarity of Gk. ἔ-φυ = Skt. *á-bhūt,* in Greek (as also in Albanian *-bi-më* [plant]: Gk. φυ-μα- and Arm. *bu-n* [stem] a.o.) the root is represented mainly in its older lexical meaning *to grow* (of plants): Timme 1935 (cf. the conclusion: "bezeichnet φύσις nicht etwa eine zauberkräftige Eigenschaft, sondern ausschliesslich die äussere Erscheinung des Wunderkrautes"); Rozhansky 1979, 64–114 (on φύσι-ς [*growth, magical quality of medical plants* > later *nature*)]; cf. OCS *bylije* (plant, medicine grass), Rus. *byl'je, bylinka* (grass); Alb. *bijë* < **bhūlium* according to Pisani, Messapic *bilia* . According to Illich-Svitych this concrete meaning goes back to Proto-Nostratic; cf. an especially clear Indo-Uralic comparison referring to plants: Finno-Ugric **pux-ti* (tree) < *pyγe-* (grow). The way the meaning of *past* comes out of this vegetative imagery is yet felt in the Russian (peasant) proverb "*vs'o bylo da byl'em poroslo*" (lit. everything has happened [was = *bylo*), and the grass [*byl'je* (grass), cf. *byloe* (past)] has grown on it) = it was long ago."

59. Starke 1990, 452; Ivanov 1999b, 288 (with references).

60. Melchert 1994, 214 (a suggestion by C.Reiss).

61. Ivanov 1998a.

62. Gamkrelidze, Ivanov 1995, 622, 639; Whittaker 1998, 144. There is no necessity to suppose a change of an ancient complete (intensive) reduplication as it is done in Frayne 1992, 21.

63. Ivanov 1999d, 216–18. If Chinese *ch'e<*kl*^{y}*ag* (Mair 1990, 45–46) is borrowed from Indo-European, its source should be a dialect that used such a non-reduplicated form.

64. Ivanov 1999d, 233.

65. Mažiulis 1988–97, 2:160; Eckert 1995, 51–65; Ivanov 1999d, 219.

66. Starke 1995, 126; Ivanov 1999d, 215–16 (see there also on the stem *tur-i-* < **dhur-*); Hawkins 2000, 103 (Karkamiš A 11 b+c, 3, §7) ; 113 (Karkamiš A , 2 §4).

67. Hawkins 2000, 112. Both Luw. and Hur. words for *road* with the stem *ḫar-* might have connected initially to the root of Akkadian *ḫarrāru(m)* (road).

68. Benveniste 1969, 240; Gamkrelidze, Ivanov 1995, 658–60 (with references).

69. Other synonymous words existed in Baltic, derived, for instance, from **wal-* (ruler) common to a part of Western IE, B-S, and Toch. and different from the noun **rēg'* shared by Italo-Celtic and Indo-Iranian. These isoglosses may point to several social units inside the area of Indo-European dialects. On Luwian cf. Hawkins 2000, 61–62, 521 with a definite formulation of the Luw.-Lyc. terminology of kingship. The HLuw. form *hasusari-* (queen), on which an opposite opinion of Starke rests, seems to be a Hit. borrowing. (As HLuw. continued the Hit. imperial tradition after the break of the Hit. kingdom, several terms had been borrowed from the vocabulary of the OHit. official documents; see above on HLuw. *ruwana.*)

70. The interchange *-l-/-r-* makes possible also a comparison to Luw. *kuwar-ša* (division). See on this a footnote in table 3 of the Supplement.

71. On a possible borrowing see Wagner 1985, 103–4. Besides a Lak correspondence mentioned by Wagner, there is a probable connection to the Adygh-Kabardinian **-kə-* : Nikolaev, Starostin 1994, 223.

72. Trautmann 1923, 348; Fraenkel 1962–65, 1219. See Hawkins 2000, 154, on Melchert's suggestion of a Luw. verb cognate to Hit. *mer-*.

73. Ivanov 1998b.

74. On this still controversial Hit. form see Melchert 1994, 96 (references). The IE term

is considered to have parallels pointing to the probable pre-Nostratic (Pan-Eurasian) origin (one of the possible words of the "language of Adam and Eve," first immigrants from Africa around 50000 B.C.).

75. Cf. Hawkins 2000, 367. This suffix (evidently containing a preserved laryngeal) is one of the many peculiarities of Luw. word-derivation making it look so different from the Hit. one.

76. The phonetic resemblance to Hur. *ašte* (woman) (with a probable N-Caucasion etymology: Nikolaev, Starostin 1994, 1094) might have contributed to the role of this stem, particularly in abstract derivatives. Meriggi 1957a, 222, who was the first to suggest the existence of the Luw. word, thought even of a Hurrian borrowing. That does not seem plausible. The reconstructed (already by Szemerényi who could not yet know the Luw. data) IE term **eso-r* (see Pisani 1951 on the suffix) may be compared as old Nostratic heritage to Altaic **as(^)V* (woman) > Tungus-Manchu **asī* (woman, the elder brother's wife), and cf. Turkic **ac^aj* > Tartar dialect *az^|a* (mother) (Starostin 1991, 206, no. 99; 198, no. 265; 216, no. 141; 292, no. 42; Ivanov 2000, 8–9). A link between Nostratic and N-Caucasian (macro) families seems also possible in this case.

77. The sound shape of the Luw. stem makes it possible to suggest the reconstruction **sw-esor* that was reinterpreted as **swe-sor* with a shift of a morphemic border after the loss of the primary noun that had been preserved only in Luw. and Gk. See also Carruba 1991.

78. As discovered by Čop 1979, 21–22, Hit. *nega-*(sister) has Uralic correspondences; to this Altaic parallels might be added. A stem might have existed in Luw.: Luw. *niya-* < **neg'ho-* in *anni-nniya-mi-* (cousin) corresponding to Hit. *anna-nega-* (the argument of the double *-nn-* [Melchert 1993a, 16] does not apply to a Sievers-Edgerton phenomenon after a juncture inside a compound); see on these words Ivanov 1999a, 39 n.56 with references.

79. HLuw. *hali-* (day): Hawkins 2000, 521. The semantic connection between (day) and (sun) has parallels in several IE dialects including NA and SA.

80. Laroche 1959, 53; Melchert 1993, 94; Ivanov 2000a. Cf. on HLuw. Hawkins 2000.

81. KUB XXXV 43 II 16–17; Laroche 1959, 147–48 (with references); Starke 1985, 144.

82. On semantic shift in the words for *right* and *left* cf. Beekes 1994; Ivanov 1999e. The evil according to Luw. and Hit. texts was connected to the bad left side (Hit. GUB-*latar*): Laroche 1959, 52; Melchert 1993a, 90. See Ivanov 1999a, 38–39, on Luw. word for *left* (*ipala-*) cognate to Toch. (B *śwālyai*, A *śālyi* [left]) and to migratory cultural terms in other families. On the symbolism of the left side in the Hit. religion see Ivanov 1976; on typological parallels cf. Ivanov 1999e; Needham 1973; Maybory-Lewis, Almagor 1992 (with references).

83. Ivanov 1985, 47–48 (with references to works by Friedrich and Puhvel who discovered the Hattic- Hit.-Gk. semantic connections).

84. Hawkins 2000, 98. See a probable connection to the Hit. and Luw. names of the moon god and the moon in the last part of the article.

85. In spite of the rare character of examples where Hit. *ḫ-* corresponds to a IE "guttural" stop in the other IE dialects (different from the large number of cases of the change *k>ḫ* in Luw.), it seems that in *ḫaršar/-n-* the whole structure corresponds to the stem of such words as Lat. *cerebrum* < **k'er(e)srom*, and the word should be cognate to it (but see above on Hit. *karawar* (horn) with a different phonetic development, and cf. the arguments against this suggestion in Nussbaum 1986, 21–22 n.4; 198).

86. Hawkins 2000, 51.The meaning of a corresponding noun in the other IE dialects is

reconstructed as *shoulder, upper part of the arm*; cf. Lat. *armus*. Pokorny 1959, 58; Toporov 1980, 68–70. (Different from the Luwian word for time, see above, the root had an initial laryngeal). One may suppose a shift: **shoulder/neck > head.*

87. Hawkins 2000, 468, 516, 526. Although the Ivriz sculpture of the god with vine-grapes has been reproduced several times, only in this book did Hawkins find the exact correspondence to the ritual HLuw. texts. The suggestion on a probable source of Gk. θύρσος (sacred sign of Dionysos) in HLuw. *tuwar(i)sa-* (vineyard) (see on the form Hawkins 2000, 254) that goes back to Gelb and Bossert may be definitely proven by these data. As Neumann (1961, 86–87) suggested, there might have existed an etymological link of the Gk. and HLuw. words also to a Gk. term (τύβαρις) for a sort of salad prepared with vinegar. This suggestion makes it possible to include the HLuw. name of the wine-god *tipariya-* (Hawkins 2000, 66 and 97) in this group of nouns. But the linguistic explanation (according to Neumann, a dialectological one) of the phonetic difference between the two forms still is to be understood.

88. KBo XIII 263 2', 7', Starke 1985, 256. As the river Euphrates (*Mala*) is mentioned in the beginning of the fragment and spatial designations like *išaruwaya* (right) (above, 38) are used in it, it seems that a description of a certain place is given there. Thus the meaning *vine* and not *wine* seems more appropriate, although the latter (as suggested by Melchert 1993a, 269) is not absolutely excluded.

89. Bo 2447 Rs.IV 16, Starke 1985, 313. In a similar text the ecstatic cry of the singers (reminding of Dionysian rites) contains the same name of the god (written without a determinative: KUB XXV 37 +XXXV 131+ 132 II 17, 19; Starke 1985, 347) repeated twice. These two names probably constitute a line of eight syllables typical of Luw. metrics that go back to IE poetry (Watkins 1985): *winiyandan/winiyandan* (-ˇ-ˇ/ -ˇ-ˇ). In a Luw. song in this same text (vs. II, 33', Starke 1985, 345) the form *tarḫuntaššinzati* (belonging to Tarhunt) is used (Oettinger 1979b, 199–200; cf. Starke 1990, 136ff.), while drinking of "wine" (Sumerian logogram GEŠTIN) is mentioned several times in the Hittite text. See on the locally restricted later HLuw. name of the wine-god *Tip/wariya-*: Hawkins 2000, 66, 97 (cf. *tuwari-sa* [vineyard?] and cf. above, note 87).

90. Gamkrelidze, Ivanov 1995, 557–62.

91. Gamkrelidze, Ivanov 1995, 1:517–18. The semantic reconstruction sketched in the book can be corroborated by Luw. data, which, at the time of writing, were unknown to the authors.

92. The term that may be related to Turkic and Mongolian words meaning *pure* seems to belong to a group of Hit. words like *nega-* that are comparable to Nostratic vocabulary and thus may present a serious argument for the "Indo-Hittite" theory (see above).

93. Melchert 1993a, 87, 120; 1994, 80, 258.

94. See Ivanov 1997 on a semantic difference between this word and other names for water.

95.Inspite of Watkins's objections the meaning seems evident and is accepted by most scholars (cf. Ivanov 1997 with references).

96. A *-r-/-l-* change may however help to compare Hit. and Luw. words. A hypothetical "Eurasian"/Nostratic parallel may be present in Aleut. *al-* (sea) (Ivanov 2000b, 11).

97. Ivanov 1957; 1987 (with references to a large literature dedicated to this exceptional Hit.-Sl. parallel). As the origin of Hit. *-u-* (corresponding possibly to a laryngeal in the position between a resonant and a stop in the middle of this word) is not clear (see above, fn. 16), it is possible also to think of a syllabic **n̥>un* with a later loss of a nonsyllabic nasal before a velar stop (in which case a laryngeal simply disappeared in this position).

98. The suffix *-n-u-* if it is not explained as a later productive morph, may point to an original *-u-* stem of the adjective from which the verb is derived.

99. Otten 1953, 110–11 n.10. Otten stressed the conclusion on a "not too close" connection between Hit. and Luw. that had a lot of old borrowings in its vocabulary. CLuw. *kummai-* (pure), HLuw. *kumali-* (pure), and **kumaza* (priest) (= Lyc. *kumaza* [priest]; Hawkins 2000, 474, 627) may be related to the stem of Hur. *Kumm-ene*, an epithet of the god *Kum-arbi* (whose residence is the city *Kumme*).

100. As a probable borrowing from a southern Mediterranean language (serving as a link to a more distant one of the Benue-Congo group) the stem might be compared to Bantu *-ntu* (people).

101. Ivanov 1999a.

102. Nikolaev, Starostin 1994, 513 (an initial *p-* in the beginning of such forms is a result of secondary development). Cf. also the name of Hattic god *Tešḫa-puna- /Zašḫa-puna-*.

103. Jamison 1982/1983.

104. Common Semitic; see Gordon 1965, 490, item N 2423 on parallels in Hebrew and Arabic to Ugaritic *šlyt*. A probable link of corresponding Semitic words to a name of a Hyksos general rendered as *Salitis* = *šallīṭ* is discussed by Loprieno 2000. A possible connection of Hit. *šall-i-* to CLuw. *salḫa-*, HLuw. *salah-* (Hawkins 2000, 241) makes the etymology of the former word controversial.

105. The Ugaritic title on the base of which Laroche deduced a corresponding Hit. term might have contained a second word of a Luw. (and not Hit.) origin. Cf. on HLuw. Hawkins 2000, 489, 630.

106. The relation between a verbal stem *to hear* with an (archaic IE) suffix in -**s-* and a derived noun *ear* in -**n-* is parallel to a formation from a different (IE) root **k'leu-* (to hear) in Toch. A *klyo-s-* (to hear), Toch. B *klyau-s-* (Skt. *śro-ṣ-a-ti*, OCS *sly/u-š-ati*, Old High German *hlo-s-ēn*), Toch. A *klots* (ear), Toch. B *klautso* (stem in *-n-*). It may be supposed that when the AfroAsiatic (Egyptian or Semitic) stem was borrowed into prewritten Hittite, there still had existed a traditional verb derived from **k'leu-* that influenced the derivational type of this loan word and its derivatives. See Starostin 1991, 143, on typological (Altaic, particularly Old Japanese) parallels to the formation of the names of the parts of the body (ear, eye) from the corresponding verb (hear, see [cf. above, 2]).

107. Starke 1990, 133, 334, 389–90; Melchert 1993, 232–33; see Hawkins 2000, 126, on HLuw. **tumanti-* (to hear). The idea of the common origin of these Hit. and Luw. words seems possible only in so far as they may go back to the same (or similar) migrational cultural term borrowed into languages of several linguistic families (including IE and Kartvelian). Cf. on the etymological links of the Hit. words for *to hear*: Gamkrelidze, Ivanov 1995, 1:793; Wagner 1985, 103 (an Egyptian and Semitic parallel had been pointed out much earlier by Forrer, 1930, 244–45). But the absence of the initial **s-* in Luw. reminds of the peculiar transformation of the IE *s_{mobile}; cf. above, 2.

108. Also, in the list above such grammatical elements were not included that can be explained by these phonetic correspondences; cf. Hit. abl. *-az*: Luw. abl.-instr. *-ati*; Hit. reflexive particle *za*; Luw. *ti*. (It can be supposed that in Hit. a final *-i* disappeared after it had caused palatalization of a preceding dental stop). Cf. Neu 1979.

109. Still, the two different Hit. reflexes of the palatalized **d'* and **t'* together with clearly non-palatalized **dh* in the same position (as in *it<*i-dhi*) are important in showing (together with the reflexes of the IE "guttural" phonemes in Luw., Ivanov 1999a) that both in Hit. and Luw. all of the three series of IE stops had been distinguished initially. That contradicts the idea of the Proto-Anatolian change to two series as reconstructed by C. H. Melchert.

Using traditional phonetic signs for the sake of general understanding, I am not discussing here the problem of a probable phonological interpretation of these series in IE and separate dialects ("voiced" as lax or glottalic, voiceless as tenuis or geminated, "voiced" aspirated as aspirated with optional voicedness/voicelessness; cf. Gamkrelidze, Ivanov 1995). Depending on the results of such interpretation, and on the view of the Sievers-Edgerton law in Hit./Luw., of the "Čop's law" in Luw. and on the accentological or tonological understanding of Hit./Luw. *Pleneschreibung*, the Proto-Anatolian phonological isoglosses suggested by C. H. Melchert could possibly be understood in a different manner.

110. Starostin 1999.

111. Neu 1974.

112. Otten 1973.

113. Starke 1985, 112ff.

114. Starostin 1999, 36–37.

115. See details in Ivanov 1998; Hawkins 2000, 60, 440, 530. On Lyc. and Mil. words for *all* (probably related to Hit. *šuwatar* (fullness) cf. Houwink ten Cate 1965, 94; Starke 1990, 467, Melchert 1993a, 29, 128.

116. Meriggi 1980.

117. Rosenkranz 1978.

118. Melchert 1994 (with rich bibliography).

119. That does not mean that the results of the comparative Anatolian grammar are not valid. As a system of formal relations it is a collection of definite facts. But their temporal interpretation may be suggested without assuming a separate Proto-Anatolian language different from Proto-IE (cf. the interpretation of the latter in the works of Trubetzkoy and Pisani).

120. Among the most important results, one might mention the discovery by A. Morpurgo Davies of correspondences to a difference between endings of the verbs of *-mi* and *-ḫi* conjugations in Hit. and those in the other Anatolian languages. But at least for mediopassive the difference may be shown to be Proto-IE (Ivanov 1981).

121. Although all the terms are conventional, I would still not like to use this one outside of the present discussion for the following reasons: there is no need to change the name of the whole family; in the new term neither Luwian nor Anatolian in general is evident; all the rest of IE goes back to this protolanguage (see above on glottochronological indexes), and it should be expressed in the term itself.

122. On the metamathematical analogy I would call a metalanguage a formal system built for a description of an object language. In comparative linguistics an object language is a system of correspondences between the languages to be compared.

123. The fifth "defining isogloss of Proto-Anatolian" according to Melchert 1994, 7.

124. Ivanov 1981, 32–33. Among other archaisms of the Hit. and Luw. verbal systems that have exact correspondences in Skt., one may also mention the ending of the 2.p.pl. imperat. in *-tana* <*ten- and a restricted group of verbs with a suffix *-r-yo-* > H- *rya-* (Hit. *etriya-*: Luw. *adriya-*; see Ivanov 1981, 32–33.). Another interpretation of a Phrygian form is possible: Thumb-Hauschild 1959, 199, § 421 IV.

125. If Lat. *aqua* is derived from the root, it is still a noun, and a corresponding verb is absent in Western IE dialects. On another etymology cf. Puhvel 1984. Hit.-Luw. *pašk-* (swallow) (Oettinger 1979, 435f.), CLuw. *paš-* (*swallow*, Van den Hout 1998, 108–9, 296), HLuw. *pa-za-*, *pa-nuva-* (*drink*?—Hawkins 2000, 394, 471) may be a trace of the IE term for *to drink* that had been inherited from the protolanguage.

126. In CLuw. the reduplication is combined with the synonymous iterative suffix *-šš-* < *-s/sk-*. In Hit. this suffix has ousted reduplication; cf. variants like *wewak-* (reduplication of Hit. *wek-* "to ask", HLuw. *wazi-* "to request", Hawkins 2000, 551; cf.Skt. *vaś-*), later *wekk-isk-*, cf. Ivanov 1981, p. 60 a.o.

127. See deep analysis of dialectal IE data: Čop 1979, 11–14; cf. Sommer 1947, 50; Greenberg 2000, 139–40, 143–44.

128. Gamkrelidze, Ivanov 1995, 310–21; Ivanov 1999c (with detailed references).

129. According to Melchert, this is a second syntactic isogloss of Anatolian. The third one is a continuation of this feature referring not to a single sentence, but to discourse at large. Melchert correctly points to the dialectal variation. As far as etymologies of syntactical elements involved in these costructions, some of them may be older than Proto-Indo-Hittite (Pedersen, 1938, was the first to suggest Nostratic parallels to a syntactic element including the morph *ma-* in Hittite).

130. As possible features of a larger linguistic league postposed possessive pronouns may be considered that occur in OHit., Akkadian and Hurrian. See also on voluntative in *-l-* in Hittite, Hurrian, and Akkadian: Wagner 1985, 94–96; Ivanov 1999d, 236–39. Ethnolinguistic adverbs/adjectives in *-l-* like Hit. *našili* (in Hittite) are also found in different linguistic families of the area.

131. On the spread of the word cf. Forrer 1930, 242; Sommer 1947, 32; Khachikian i985, 54 (Hur. as the source for Hit.; it seems possible for the form in *-n-* if Hur. *-rl-* had come from **-r-* + *-n-*). But see on possible Northeastern Caucasian parallels Nikolaev, Starostin 1994, 312. (East Caucasian languages could not have been the original source for such ancient terms attested at an early period as Egyptian words; if the connection is accepted, it is necessary to suppose an early spread of Egyptian cultural terms in Hur. and NA and SA, as well as in Eastern Caucasian). Early Egyptian contacts to the southern coast of Anatolia, which was a place from which wood had been brought into Egypt, might be interesting from this point of view. Influences of Egyptian art and symbols are found in the art of Anatolia of the Bronze Age; an Egyptian model was present at the invention of a probable early prototype of Luw. hieroglyphic writing into which some Egyptian hieroglyphic symbols like *anh* and a winged sun-disc had been borrowed. (Some of comparable "Proto-Hieroglyphic" signs are found in the seals of the documents from Old Assyrian trade colonies in Cappadocia at the border of third and second millennia B.C.).

132. Probable loss of a noun based on **dem-* (to build) has become definite for Southern Anatolian since the verb *tama-* (to build) (< **de/om-o- = t'e/om-o-*) has been found in HLuw.: Gamkrelidze, Ivanov 1995, 645 n.3; on the verb cf. Hawkins 2000, 90.

133. Already in the first issue of his book on the decipherment of Hit., Hrozný spoke of Hit. and Lyc. connections to Hurro-Urartean and [North-] Caucasian: Hrozný 1916, IX-X, 27–28 n.3, 57–59. Cf.Wagner 1985, particularly 37 ff.

134. Rosenkranz 1978, 113. Not only can Anatolian as a whole be understood in this way, but each of the dialects inside NA and SA can also be viewed not as an isolated item with its "protolanguage" but as a part of a system of isoglosses shared with other languages, as a fragment of a linguistic continuum, which also explains features of Pal. and Lyd. shared with SA, as well as a number of Hit. borrowings in HLuw. I have also found views similar to mine in a recent publication by Watkins: Watkins 1999, 11. As to the spread of Anatolian innovations to Greek dialects mentioned in this contribution (as well as in some other works by Puhvel and Watkins), the case of the *-sk'e/o* iterative verbs seems particularly interesting. These are found not only in Northern and Southern Anatolian (in the latter mixing with the

synonymous *-s-* type probably for phonetical reasons) but also in Ionic Gk. and in Kartvelian (non-IE, but Nostratic and thus distantly related) languages; see above on a form in *-sg-* in Svanetian. A dialectal spread of nominal endings may be supposed for spatial secondary cases in Northern Anatolian and Kartvelian (on the contact of IE and Kartvelian case systems see also Machavariani 1966).

135. Thus, according to Benveniste (1962), an archaic ending of the IE accusative in *-u* is reflected in those pronominal forms that Melchert considers one of the Proto-Anatolian morphological isoglosses.

136. A particular importance given tongues and their magical images in Hit. and Luw. rituals makes it possible that the ancient IE term (preserved in most dialects including Toch.) was ousted due to a taboo and substituted for by this new euphemistic word.

137. It is also possible that such important Hit. terms as *ara* (right, law) and *arawa* (free [of taxes]) were borrowed into SA, which may explain the use of these terms in Lyc. (Laroche 1979).

138. Since the Hit. word for *time* shows a link to the IE word for *measure* and the name of *moon*, it seems possible to suggest a similar semantic chain uniting Luw. *arai* (time), Toch. B *yarm* (measure), Gk. άρμονίη, Hit. and Luw. *arma* (moon) (cf. an ON kenning *ár-tali-* [year-counter = moon]). The source of the innovation seems to be Luwian.

139. Blažek 1999, 203 (with references).

140. Theoretically possible is also another (westerly) place for their homeland (for instance, in the Balkans). In that case the Greeks (and Phrygians) did not move far from this hypothetical place, but Proto-Armenians and Indo-Iranians (Aryans) went in the eastern direction. The arguments against this alternative suggestion are given in Gamkrelidze, Ivanov 1995.

141. Because the main topological difference is between movements, on the one hand, that do not change the relations between the elements and those, on the other hand, that lead to breaks (catastrophes), the dispersal of Eastern IE may be viewed as one of decisive events in the early prehistory of the family.

142. This idea goes back to Forrer and was explored by Palmer and many other scholars. Among many possible arguments for this point of view, one can cite the toponym *Parnassa-* (HLuw. *pa+ra/i-na-sa*, Hawkins 2000) in Cappadocia that is surely identical to the famous Gk. toponym. The name is derived with a Luw. suffix from a term for *house* (> *temple*) common to Luw. and other NA and SA dialects (see above).

143. Gamkrelidze, Ivanov 1990–91.

144. The Hit. words for the 100-word and 200-word lists were chosen (with some minor changes) according to Rosenkranz 1978, 103–7. The standard list of 100 and 200 words as given in the lexicostatistics and glottochronology according to Swadesh cannot be applied in full to Hittite and Luwian since we do not know the phonetic readings for some important terms always written in logograms or Akkadian heterograms, and some terms from the list do not have accepted Hittite and Luwian translations. That explains the redution of the 100-word list to 72 items in table 1a and of the 200-word list to 166 items in tables 1a and 1b.

145. Probably a Semitic borrowing; see the Akkadian word *abru*.

146. It can be interpreted as a Luw. borrowing becouse of the absence of a nasal.

147. Inter-Anatolian cross-influence (marked as IA in the following in brackets in the text).

148. If not a Semitic borrowing; see footnote 104 above.

149. Melchert 1993a, 157 (arguments for not accepting the meaning *not* in this [first] ritual of Kuwatalla).

150. In this function the grammatical morph is not present in Hittite.

151. Starke 1990, 109 versus Melchert 1993, 56; 1994, 116. If it is a a Luw. borrowing it seems that a borrowed Luw. form in *-ša* has been reinterpreted as a Hittite one in *-ešša*(*r*).

152. Starke 1990, 134, versus Melchert 1993, 166. The stem is shared by both the dialects if the meaning in this controversial text of a ritial is *take*: Melchert 1993, 121–22; 1994, 238, 245,274; Meriggi 1980, 261.

153. Watkins 1993, 269; Melchert 1994, 241, 263.

154. In Hittite the root has another meaning referring to speaking. In the rest of IE it refers to intellectual activity in general and to the memory; the connection between *seeing* and *knowledge* is attested in the root **weid-* as well.

155. See above, section 2, on the correspondence: Hit. *š* : Luw. *t*.

156. On these words in SA and NA see Hawkins 2000, 65.

157. OHit. also renders the notion of a direct object of neuter gender.

158. Old English *dubbian* (strike), Low German *dubben* (hit), Germanic **dub-ila-* (peg) (with probable Slavic correspondences): Pokorny 1959, 268; Watkins 1985, 14; Kluge-Seebold 1999, 197—A Luwian-Germanic-(Slavic-) Greek isogloss.

159. In Hittite there is another first element of this ancient compound, but it is also Indo-European

160. A specific semantic development characterizes only the Luw. verb.

161. A similar form may be seen in Lith. *kāras* (war), Old Persian *kara* (army) if corresponding Gk. and Celtic forms are not original but borrowed from a satem (probably Iranian?) dialect. From this point of view a relation to Luw. *ku*(*wa*)*la-* (army) is also possible, but the variation *-r/-l-* may be old and not necessarily speak for an (Indo-)Iranian origin of the form.

162. If borrowed, in both dialects from cognate sources.

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ON THE QUESTION OF THE ANATOLIAN ORIGIN OF INDO-HITTITE

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In the past fifteen years several proposals have put the homeland of the original speakers of the Indo-Hittite (IH) languages in Anatolia. I propose in this paper to evaluate these proposals. Any solution to the problem of the homeland of Proto-Indo-Hittite (PIH) must involve both linguistics and archaeology. As a linguist, I start from the linguistic data. It is well to remember, however, that the problem is posed by linguistic data, and any solution not fitting the linguistic data is unacceptable. We need to account for the linguistic facts of the extraordinary spread of Indo-Hittite languages, the reconstructed vocabulary, and the relative chronology of the division of the language family.

At the dawn of recorded history, the languages of this family were found over most of Eurasia. They extended from Ireland to India and western China, from the Baltic Sea to the Mediterranean and across into Anatolia. While history dawned at different times in different places, this distribution is certainly ancient. The fact that these languages have been demonstrated to belong to a genetic language family indicates that all these languages evolved from a single fairly coherent language. In the absence of mass education and government policy, the area that can be covered by a coherent language without substantial differentiation is limited. That means that the IH family underwent an extraordinary expansion across Eurasia at a time before the existence of large cities and empires and, at least initially, when trade was conducted through exchange networks rather than through established trade routes. Any prehistory of Eurasia that does not account for that expansion is woefully inadequate.

Ideally, we should try to discover the time and place of the beginning of the expansion and to offer some explanation for that expansion. The original area certainly has to have been much smaller than Eurasia. The amount of dialectal variation that we find in a language not artificially reinforced by a government or an education system varies, depending on the topography. Mountainous areas have intense variation, as do coastlines (Nichols 1992: 233). Forested areas have less but still substantial variation across areas not connected by rivers. Open plains have better communication and less variation. Nichols (1992: 15) notes that throughout known history the Eurasian steppe has had one basic language at a time. She does not, however, exclude the possibility of dialectal variation in that basic language. While we would like to reconstruct PIH as a pure dialect without variation, this is very unlikely. We should certainly not look for any area larger than the Russian steppe, and if it is that large, it should be an open plain. If we place the homeland of PIH in a mountainous region, it could be quite small. James Mallory (1989: 64) estimates that to have a coherent language without significant dialects, we should not aim for a territory larger than Germany or Poland. Actually both Germany and Poland have old and significant dialectal variation. In fact, Kashubian in northern Poland is best considered an independent language. However, we are unlikely to get any closer than an area that large.

In discussing the vocabulary of the parent language, the operative word is *reconstructed*, not *shared*. In the process of proving that two languages are genetically related, linguists must demonstrate that there are regular relationships between the sound segments (vowels and consonants) of the two languages such that regular sound changes can derive both languages from the forms of a parent language. To be a properly reconstructed item in the parent language, a word must have cognates in both languages, and it must be demonstrated to have undergone all the regular sound changes differentiating the two languages. Such a word must also have similar enough meaning in the two languages to warrant belief that the two meanings evolved from a single vocabulary item. Linguists typically start with semantically stable items to establish the formal relationships and only then look at words with formal agreement but varying meanings. For Indo-Hittite, there are hundreds of words or roots for which we are reasonably sure of both form and meaning.

When we apply the comparative method, we compare items in daughter languages. We reconstruct items based on agreement among those languages. This means that the items we reconstruct had to be in the parent language at the time and place of the separation. It also means that we cannot go back any further than that time and that place—at least not systematically. The reconstructed items in the language are those that existed at the last place and time when the speakers of the protolanguage were together in a single area and constituted a reasonably unified linguistic community. That, for a linguist, is the homeland. If we go back any further in history, we are talking about the history of culture and hoping to relate that to the history of language.

The question of the relative chronology of the division of protolanguage brings us to the Indo-Hittite hypothesis. In its weakest version, the Indo-Hittite hypothesis says that the first division in Proto-Indo-Hittite (*proto-* indicating the posited parent language of the family) was between Proto-Anatolian (PA) and the rest of the family. Originally the whole family was called *Indo-European*. For people who use the term *Indo-Hittite*, the non-Anatolian branch is called *Indo-European* (IE), and the reconstructed parent of that branch is called *Proto-Indo-European* (PIE).

Stronger versions of the Indo-Hittite hypothesis claim that there were a significant number of common innovations in Proto-Indo-European before it began to break up into the various subfamilies of Indo-European. This would mean that reconstructed PIE would be a slightly different language from PIH. It would also allow for the possibility that a considerable amount of time elapsed between the initial division and the break up of PIE into its subdivisions. Because the time of the division is different, the place of the division may also be different. Thus, arguments for the homeland of PIE, based on PIE data, may not directly apply to the discussion of the homeland of PIH.

My approach in this paper differs from previous approaches in two ways. First, I take the Indo-Hittite hypothesis, at least in its weaker variant, very seriously. I will make a systematic distinction between vocabulary items reconstructible for Proto-Indo-Hittite and those reconstructible for Proto-Indo-European alone. This is a reasonable precaution when we are discussing Anatolia as a possible homeland. The second differing aspect of this study arises from the fact that I will use no environmental terms for my arguments. I will limit myself to cultural terms. This is because environmental terms are systematically unreliable at the level of Indo-Hittite. For the level of Proto-Indo-European such terms might theoretically be useful, but they have proven not to be so.

The reason that environmental terms are systematically useless for PIH is that the first division in PIH is binary. Suppose we have a language family L with two branches, A and B, which together occupy more territory than we could reasonably expect to have only a single language. We might try to use environmental terms to select one of the two territories as the original homeland. If both A and B have a word for *oak tree*, and this word can be derived from a proto-form in L, then we can assume that the original homeland had oak trees. However, since A and B have an inherited word which means *oak tree*, oak trees must exist in the territory of both A and B, so the term is of no use. If a term for *oak tree* exists in A, but not in B, then we cannot determine whether the word was in the protolanguage L. If a form from L shows up in A with the meaning *oak* but in B has the meaning *birch*, we cannot decide the meaning in L; again, it is of no use. Similar arguments would apply to any environmental term.

Because there is no guarantee that the initial division of the PIE branch was binary, we might have better luck finding environmental terms in PIE. A systematic problem still exists, however, but perhaps not a systematic impossibility. If there were an animal or plant unique to the original homeland, we would be guaranteed to miss it. When the language expanded to areas where the plant or animal did not exist, then the term would either go out of use or be applied to some other plant or animal. Thus, we either could not prove that the word was PIE, or we could not arrive at a reconstructed meaning. To prove that a term is PIE, a reconstructible form must occur with essentially the same meaning in several IE languages. This means that a term with reconstructible form and meaning must refer to an environmental feature with broad distribution, which lessens its value for locating a precise place of origin. At best, we could expect only that a given term which is arguably PIE could be used to eliminate part of the territory where IE languages are found. A large set of terms might allow us to eliminate a large part of the territory occupied by historical IE languages. In practice this is not the case. James Mallory (1989: 117, 163) is one the most careful scholars who has applied this methodology. In Europe, the only places that he could eliminate on the basis of environmental terms alone were the treeless parts of the Russian steppe and Greece.

If we limit ourselves to cultural terms, then we obviously cannot make any direct arguments for the place of origin of Indo-Hittite. We can identify candidate cultures, and we can use datable cultural terms to limit the time of the first dispersal of the Indo-Hittites. We use positive evidence for cultural items to establish a *terminus post quem* for the first division of the protolanguage. With considerably more danger of error, we can use the absence of important cultural terms to establish a *terminus ante quem* for the division. For instance, I will argue that the existence of a reconstructed term for *yoke*, combined with the absence in Anatolian of a whole set of terms relating to wheeled vehicles, could place the division of PIH into PA and PIE between the introduction of ox traction and the introduction of the wheel. Since languages can and do spontaneously lose or replace words, positive evidence is always much more reliable than negative evidence. The positive evidence indicates that the division between PA and PIE took place after the introduction of ox traction (c. fifth millennium B.C.) and that the dissolution of PIE took place after the introduction of wheeled vehicles (c. late fourth millennium B.C.). It is negative evidence (the absence of terms for wheeled transport) which indicates that the division of PIH took place before the introduction of the wheel. We should look for evidence from other sources to confirm that. I will argue that we may find such other evidence in the development of putative IE cultures in Europe. It is broadly accepted that IE speakers played a prominent role in the Yamnaya and Corded Ware cultures. These cultures occupied a territory much too large to allow for a unified language; thus, as the cultures differentiated, so would have their language. This would establish a division within the PIE branch of PIH and entails an earlier differentiation between PIE and PA. The younger of these cultures, the Corded Ware culture, began in the late fourth millennium. We thus have a second line of evidence that could place the *terminus ante quem* for the division of PIH in the second half of the fourth millennium.

Are we limited merely to proposing candidate cultures for the original homeland? In the general case, I think we are. There is no one-to-one correlation between culture and language, and all we have are cultural artifacts. In the case of Indo-Hittite, however, we may be able to make reasonable suggestions. Luckily, the number of candidate cultures is restricted, even in the broad area where Indo-Hittite is found. The fact of the extraordinary spread of Indo-Hittite languages adds to our good fortune in this search. While culture can spread without language, language is an extremely important cultural artifact unlikely to spread by itself. We should be able to detect in the archaeology the kind of cultural contacts that would allow for the spread of language. Furthermore, the extraordinary spread of Indo-Hittite should be associated with some set of important changes for which we can expect to find evidence in the archaeological record.

Vocabulary items that are reconstructed for PIH and PIE reflect what Sherratt (1981) called the secondary products revolution. This involves the use of animals for labor, for transportation, and for such items as milk and wool. The use of animals to increase production, and in particular the use of animals to produce food and clothing without the necessity of killing them, had the potential to add a considerable amount of wealth to human society. The use of ox-drawn plows could allow for the use of land that was previously unusable. Because a few men could take care of a large number of animals, the more animals the greater the wealth. A society that adopted this system ahead of its neighbors would have an advantage both in wealth and mobility. It could expand rapidly into both unexploited geographic and economic areas.

All this will, I hope, become clearer as we look at concrete data. I will first survey the proposals that I will evaluate.

Thomas Gamkrelidze and Viacheslav Ivanov (1995), in *Indo-European and Indo-Europeans* (the English translation of a 1984 book in Russian), use archaeological and linguistic evidence to place the original homeland south of the Caucasus in Anatolia, in the fourth or fifth millennium B.C. They describe the steppe north of the Caucasus and the Black Sea as a "secondary" homeland of Indo-Europeans (1995: 836). However instead of postulating a geographically simple migration of most of the Indo-Europeans through the Caucasus, leaving the Anatolian Indo-Europeans behind, they present a picture of the migration patterns of the various tribes that can fairly be described as confusing (cf. their map on 850–51).

Colin Renfrew (1987), in *Archaeology and Language,* uses only archaeological data for prehistory. He starts from the premise that most of the population spread within Europe can be traced to the spread of farming, which supported an increase in population. Farming was introduced into Europe in Greece in the seventh millennium B.C.—almost certainly from Anatolia. He suggests that the language of these initial farmers spread as they spread, through what Ammerman and Cavalli-Sforza (1973) call demic diffusion. As children of farmers grow up, some may stay on their parents' farm, but others will move in random directions to find new usable farmland. This can eventually result in the spread of population over an entire continent.

Robert Drews, in his 1989 book *The Coming of the Greeks*, places the homeland of the Proto-Indo-Europeans in the southern Caucasus, but much later than do Gamkrelidze and Ivanov. He associates the spread of Indo-Europeans with the spread of chariot warfare and puts PIE unity as late as 1900 B.C. In a later article (1997) he differentiates between Proto-Indo-European and Proto-Indo-Hittite.

Using this distinction, Drews presents a variant of Renfrew's proposal. He suggests that the farmers who crossed from Anatolia to Greece in the seventh millennium B.C. were speakers of an Anatolian Indo-Hittite dialect.

Sherratt and Sherratt (1988) provide an interesting variant on the Renfrew proposal. They suggest that a pre-Indo-Hittite language did originate in the farming belt of the Middle East and spread into Anatolia, as well as perhaps into southeastern Europe. They then suggest that a koiné developed as a trading language around the western part of the Black Sea in the third millennium B.C. and that this language spread by means of creolization and as a trade language.

I will consider only one alternative to the Anatolian solutions. James Mallory's 1989 *In Search of the Indo-Europeans* uses an impressive array of archaeological material, integrated with a sound knowledge of Indo-European scholarship, to place the homeland of the Proto-Indo-Europeans in the steppe north of the Black and Caspian seas, sometime between the late fifth and early third millennium B.C. The specific people in the steppe were of the Pit-Grave (Yamnaya) culture or its immediate predecessors, the Khvalynsk and Sredny Stok cultures. Mallory presents archaeological evidence for the possible spread of people and cultures from that area. Mallory's work is really a continuation of the work of his professor, Marija Gimbutas. Her collected works on this problem have been published as *The Kurgan Culture and the Indo-Europeanization of Europe* (1997). (The label "kurgan" comes from the Russian word for the characteristic burial mound in this culture.) I think that the Mallory-Gimbutas hypothesis provides the best fit for the PIE data, but neither of them seriously considers the possibility of a difference between PIE and PIH.

I will concentrate in this paper on a few cultural items possible to trace in the archaeological record. The items demonstrably PIH are words for *wool*, *horse*, and a group of words associated with ox traction: *yoke*, *harness pole*, and possibly *harness*.

PIH Cultural Terms

(1) HwlHn- (wool)

> Skt. *ūrnn̥ā* Av. *varəna*, Gk. (Dor) *lânos*, L. *lāna*, Lith. *vìlna*, S.C. *vű̄na*,
> OE *wulla*, Hit. *hulana-*.

All of the forms can apparently be phonetically derived from a stem **Hwl̥Hn-*. There have been some questions raised about the presence of two laryngeals in the stem. Hittite is the only language that indicates a word-initial laryngeal. This raises a question about the lack of a prothetic vowel in the Greek word. Initial laryngeal-consonant clusters in Greek often result in an inserted vowel. Beekes, one of the enthusiastic proponents of this hypothesis, is only willing to say (1988: 71) "It seems that a laryngeal was vocalized before *wl-* (and *wl̥-*)." He qualifies both his example etymologies with "if." Similar problems might be identifiable with the relatives of

Hittite _hurki_ (wheel). Gamkrelidze and Ivanov (1995: 623) relate this to a family of words that began with *_Hw(e)r-_ and meant _rotate_ or _turn_, including *_Hwer-t,_ and _Hwer-ǵh-_. Pokorny (1994: 1,157) relates Greek _hratánē_ (stirring spoon) to this family of words. Another possibility is Greek _rhembomai_ (rove, roll about), and _rhombos_ (magic wheel), which may be from an n-infixed form of *_w(e)r-g_^{w}-. If this is correct, then there is something yet to be explained about the relationship between initial _h_ in Hittite and Greek prothetic vowels.

Adams (Mallory and Adams 1997: 648) solves the problem by postulating a metathesis to change a Proto-Hittite form *_wlHn-_ to *_Hwln-_. He leaves the initial laryngeal in _hurki,_ however.

Gamkrelidze and Ivanov (1995: 178) suggest that the original form could be *_Hwl̥n-,_ with the word-initial laryngeal influencing the syllabic _l_ to form the long syllabic that is reflected by the acute intonation in Lithuanian and the long _ā_ in Greek and Latin. If this were a phonetic process in PIH, then we could not postulate an initial sequence *_Hwl-_ in the Greek words _lákhnē_ (woolly hair, down), and _lákhnos_ (wool), and could not relate them to other words for wool and hair. We would also expect to find the reflexes of long syllabic _r_ in the zero-grade of words for (turn), which they (1995: 623) postulate as *_Hwer-th_ and _Hwer-ǵh._ We do not find this reflex in Lithuanian _vir̃to_ (turned over), or Sanskrit _vr̥ktá_ (twisted). We would also expect a long _ū_ in Sanskrit _ubhnāti_ (ties), and Avestan _ub-daēna-_ (made of cloth), which they (1995: 499) derive from a root *_Hw-_.

Tischler (1984–: 279) likewise argues that there was no second laryngeal after the _l_ but provides no evidence. Melchert (1994: 55–56) argues for the presence of the _H_ after the _l_ at the time of an assimilation of *_ln_ > _ll,_ since this assimilation was blocked in _hulana_. He is unsure whether the vowel after the _l_ is purely orthographic or a later insertion. This obviously serves as an argument against both Tischler and Gamkrelidze-Ivanov.

Overall, the evidence for a laryngeal after the _l_ is about as good as we can get, without an overt _h_ in Hittite. (Only one laryngeal shows up as _h_ in non-initial position in Hittite, and then not in this environment.) This includes the reflexes of "long syllabic l" in Greek, Latin, Sanskrit, and Balto-Slavic, as well as Melchert's argument for Anatolian.

The best overall solution to the problem of the presence of an initial _h_ in Hittite coupled with the absence of a prothetic vowel in Greek is to accept the possibility that laryngeal plus *_w_ did not always condition a prothetic vowel in Greek. Initial _h_ in Hittite can correspond to an _o_-coloring or an _a_-coloring laryngeal. The _a_-coloring laryngeal does condition epenthesis in Greek, as in _áēsi_ < *H_a_weHti_ (blows) compared to Hittite _huwant-_ (wind). There is, as far as I have been able to determine, no reliable example of a prothetic vowel in Greek that can be traced to an initial *H_o_w-_. Because it had a rounding effect on an adjacent vowel, H_o may have shared enough characteristics with _w_ that it could easily be absorbed by _w_.[1]

(2) *HEḰWOS (HORSE)

> Skt. *aśvas,* Av. *aspa-*, Gk. *hippos,* Mycenaean Gk. *iqo,* L. *equus,* Old Irish *ech,* OE *eoh,* Tocharian *yuk,* Old Lith. *ešva* (mare), HLuv. *á-zú-*(*wa/i*). CLuv. *? az-zu-wa-an-za* [dat. loc. pl.], Lycian *esbe.*

This is a concrete word. In this set the only phonological problem is the initial *hi-* of Greek *hippos.* The possibility that the *i* might be phonologically explainable in Mycenaean is offered in Mallory and Adams (1997: 274).

(3) *JWGO- (YOKE)

> Hit. *jukan,* Skt. *yuga-*, Gk. *zugon,* L. *iugom,* OCS *igo,* OE *geoc,* Welsh *iau,* Lith. *jungas.*

In these forms the only deviation from the basic shape of the stem is Lithuanian *jungas,* which has the *-n-*infix from the verb stem. This root is well integrated into PIH morphology. There is a verb root having an aorist stem **jeug-/jug-*, a present stem **juneg-/jung-* (join, yoke) (e.g., Skt. pres. *yunákti* < **juneg-ti,* aorist mid. *áyukta* < **éjug-to.*) Benveniste in his (1935) discussion of PIE root structure, claimed that original roots had only two nonvocalic elements. The third was always a suffix or *élargissement.* The *-n-*infix for him was always an original suffix. In this system, the "vowels" [*i*] and [*u*] are always the vocalic realizations of the glides /*j*/ and /*w*/, which count as consonants. The primary root in this case would be **j*(*e*)*w-*. In this case, the original root is actually found in Sanskrit **ju-* (unite) in the verb *yu-té* (unites), and, with a nominal suffix *-tis,* in the Lithuanian word *jautis* (ox, i.e., the yoked animal). I include this here to indicate just how sound the PIH status of this word is. It is not a borrowing.

The fact that *jugom* was a native word, however, does not imply that the PIH speakers invented the yoke. It merely indicates that in borrowing an instrument they applied descriptive terms from their own language to name it. We will see the same thing for the words for *wheel.*

(4) H(E)IHS- (HARNESS POLE)

> Hittite *hiss*(*a*)-, *hess*(*a*)- (harness pole), Skt. *īṣā* (harness pole for carriage or plow), Av. *aēša-* (plow), Sln. *oje* [gen.] *ojesa* (harness pole).

We have trouble reconstructing a concrete word here. The Hittite word, considering the attested spelling with *e,* is probably best considered from **HeiHs-* and as such is a formal match with the Avestan word, which could also come from **HeiHs-*. Pokorny (1994: 298) lists them under **ei-, oi-* (stange).

(5) DHWRH- (HARNESS)

> Several sources, e.g., Gamkrelidze and Ivanov (1995: 624), Mallory and Adams (1997: 508), Mayrhofer (1992–: 794), suggest linking Hittite *tūrija* ([verb] harness), Sanskrit *dhūr, dhúram* (yoke, load, harness shaft), Tocharian *tursko* (draft ox), and Greek *thairós* (pivot of a door or gate, axle of a chariot). Multiple ambiguities arise in these forms. The Sanskrit and Greek could come from **dhrH-*, while the Hittite and Tocharian could begin with any dental stop.

Ox Traction

I have not found any study dedicated to the invention and dissemination of the yoke. However, the combination of words for *yoke*, *harness pole*, and *harness* indicate the existence of animal traction. This should involve the traction of plows or vehicles. In both the south of Russia and in Mesopotamia, there is evidence that sledges were used as vehicles before the introduction of wheels (Piggott 1992: 16), but there is no evidence to place these vehicles before the fifth millennium B.C. There is no unambiguous evidence for plows pulled by oxen before the fourth millennium B.C., but they almost certainly go back to the fifth. Kushnareva (1997: 172) even suggests a sixth millennium date for plows in the southern Caucasus and in the Near East in general. Along with indirect evidence from sophisticated agriculture, she suggests that a piece of antler found at Aruxlo I could have been part of a primitive plow. Aruxlo I has uncalibrated carbon 14 dates of 4770 +/-60 and 4815 +/- 60 B.C. (Kushnareva 1997: 22).

Initially, vehicles were much too heavy to be pulled by horses, and oxen were necessary. Sherratt (1981: 288) presents a map of the distribution of ox traction as of 4000 B.C., with its subsequent expansion. He shows the early area of ox traction to stretch from Mesopotamia to the area south of the Caucasus by 4000 B.C. It then spread through the Caucasus, across the Russian steppe, and into the upper Balkans and central Europe. This conforms to what Gimbutas has to say about the spread of the yoke and the plow. She cites the existence of copper figurines of yoked oxen in the southern Caucasus dated to about 3700 B.C. (1970, 1997: 82). From a grave site in the steppe north of the sea of Azov (Kamennaya Mogila, near Melitopol'), which she dates from the second half of the fourth millennium B.C., she presents (1980, 1997: 273) a line engraving that seems to represent yoked oxen pulling a vehicle (without indication of wheels).

However, if Sherratt is correct about the route, the spread may have taken place earlier than he thought. In the Tripol'e-Cucuteni culture at the western end of the Russian steppe, archaeologists have found more than fifty ceramic vessels in the shape of sledges, some of which have the heads of paired oxen attached to the front (Gusev 1998). One of them (without oxen) is attributed to middle Tripol'e, which would be in the second half of the fifth millennium. The great majority are found in the Nebelevsk-Tomashivka, Sushkivka complex, which Gimbutas (1997: 43) labels Cucuteni B3-Tripol'e B III_3. She gives no carbon 14 dates for this complex, but dates on both sides (1997: 47) would make it around 4000 B.C. The earliest indirect indication of ox traction is changes in the bone structure of the legs of oxen attributed to the stress of traction. Mateescu (1975) found such evidence in ox bones from the early fifth millennium B.C. on the lower Danube. Davis (1987: 162) withholds judgment on this evidence, citing the lack of experimental controls.

The Horse

No internal linguistic evidence is available to determine whether the word **Hek̂wos* referred to domestic or wild horse. The argument that the Proto-Indo-Hittites used domesticated horses is based on the fact that the domesticated horse is extremely important in the mythology and ritual of the oldest PIH societies. A survey of these arguments is found in Gamkrelidze and Ivanov (1995: 465–75). Although the field of comparative mythology does not have very rigid standards of proof, this argument is as solid as any it offers.

Throughout all the period that interests us, wild horses were found in the Eurasian steppe and across much of Europe. According to Vörös (1981) there are no traces of wild horses in the Balkans or in the Danube valley in the Neolithic. They had begun to filter into the Carpathian plain from the northwest and begin to appear in the upper Balkans in the Eneolithic (fifth millennium).

Ample evidence exists of horses in the Kuro-Araxes culture of the southern Caucasus in the fourth millennium B.C., and they are found at two sites in the fifth millennium (Kushnareva 1997: 173). Piggott (1992: 38) reports the existence of wild horses in the fourth millennium B.C. south of the Caucasus and in northwest Anatolia around *Eskişehir*. Bökönyi (1987, 1991) argues that the animals found south of the Caucasus in the fourth millennium were domesticated horses introduced from the Pontic steppe. Piggott's report about the find in northwest Anatolia is apparently based on a personal communication from von den Driesch to Juris Zarins (Zarins 1986: 180). Zarins identifies the specific site in western Anatolia as Demirçi Hüyük. Groves (1986) simply says that "evidence for the presence of wild horses in Southwestern Asia is not very conclusive either, although Boessneck/von den Driesch (1976) claim to have identified wild horse bones from fourth millennium deposits in Anatolia." The bones identified by Boessneck and von den Driesch are the ones claimed by Bökönyi to be domesticated horses. Russell and Martin (1998) report the presence at Çatalhöyük West (c. 5000 B.C.) of several bones "belonging to large equids, which is likely to mean Equus caballus." Colin Renfrew, at this conference, verbally confirmed the existence of horses at Çatalhöyük from as early as the seventh millennium.

Nevertheless, all claims for early domestication of horses are controversial. Petrenko (1984: 70) asserts that the preponderance of young animals with milk teeth at the Neolithic-Eneolithic site of Mullino indicates domestication of the horse for purposes of meat production, but the small total number (twenty individuals) may raise doubts. Not only Gimbutas and her followers, but Sherratt (1981, 1983b), Piggott (1993: 43), and Bökönyi (1978) accept evidence from late fifth millennium steppe settlements like Dereivka to indicate horse riding and domestication. Anthony (1991a) provides a summary of the evidence and an account of how it may have happened. The evidence includes the remains of a large number of horses at a small settlement (fifty horses for three houses), ritual burials of horses, and antler pieces of a size and shape that could have been used for bridle bits. But Anthony's demonstration of wear on a stallion's teeth that could be the result of using the antler bits has recently been called into question by the stallion skull's radio carbon date of c. 2900 B.C. (Mallory and Adams 1997: 276). Levine (1996) is careful to distinguish between horse taming and riding, as opposed to husbandry, which is true domestication. She accepts the evidence for horse riding from Dereivka but sees no conclusive evidence of husbandry. Whittle (1996: 134) is even more critical, accepting Dereivka as evidence for only limited horse riding.

The doubts about domestication at Dereivka were raised by Levine's (1990) study. She examined the still-available horse bones from Dereivka and found that the ages of most of the horses represented clustered between five and eight years old. This, she argued, is too old because animals raised for food would likely have been slaughtered soon after achieving near-adult growth, roughly between one and two years. Moreover, she points out that working horses in modern Mongolian societies are kept until they are fifteen to sixteen years old. The lack of older horses at Dereivka led her to the conclusion that the horses at that site were hunted.

Levine's conclusions may not be justified. Petrenko (1984: 69–110) found similar distributions at sites that almost certainly had domestic horses. He studied sites along the middle Volga and west of the Urals. At Iron Age sites and at sites from the first millennium A.D., Petrenko's data show that less than 2 percent of the horses with determinable age lived past thirteen years, while slightly more than 70 percent died between three and nine.[2]

Horses appear in the upper Balkans and the Danube Valley in the late fifth millennium. Gimbutas (1979, 1997: 240–68) associates this with the spread of domestic horses into Europe with the expansion of the steppe (kurgan) culture, and therefore with the spread of Indo-Hittites. If indeed the bearers of the steppe culture brought horses with them in these expansions, then the horse was at least domesticated, if not ridden, before the beginning of the fourth millennium. Vörös (1981), however, suggests that wild horses may have been drifting west from the steppe at that time.

Fourth millennium evidence is similarly controversial. Gimbutas (1997: 273) cites a line drawing of a man on horseback, which she says is attributed to the second half of the fourth millennium. Häusler (1994) points out that this drawing cannot be securely dated. Possible bronze cheek-pieces for bridles are found in the Maikop culture of the northern Caucasus. Evidence that these may indeed be cheek pieces came from microscopic examination. Remains of skin were found on them (Munchaev 1975: 390). The dating of the Maikop culture has been controversial, but it seems to have begun early in the fourth millennium. Recent carbon dates taken from bones at Galiugai, a settlement of the Maikop culture (Kushnareva and Markovin 1994: ftn. 172) indicate for Galiugai I 4,480 +/-70 BP, for Galiugai II 4,600 +/- 80 BP, and for Galiugai III 4,930 +/- 120 BP, giving a calibrated range of 3730 +/- 131 to 3165 +/-90 B.C. A general upper limit for the Maikop culture is provided by radio carbon dates from the burial ground at Ust'-Dzhegutinskii. These burials are supposed to relate to a period immediately following the period of the Maikop culture. The calibrated dates range from 2755 +/- 319 B.C. to 2465 +/- 335 B.C. (Kavtaradze 1983: 107–8). However, in the area north of the Kuban river, the Novotitorovka culture is thought to follow the Maikop culture, and that culture has a calibrated carbon date of c. 3100 B.C. A general range for the Maikop culture would seem to be 3750–2750 B.C., with the possibility that it ended earlier in some parts of the region. No specific dates are connected with the cheek pieces. The horses in the Kura-Araxes culture are widely considered to have been domesticated. Kushnareva (1997: 193) argues that they were used as pack animals, but the only evidence seems to be that they appear where they were not found before, associated with sites developing transhumance of sheep.

In the third millennium we have Anthony's evidence of bit wear on the skull from Dereivika, with a carbon 14 date of c. 2900 B.C. In central and western Europe the Bell Beaker culture, dated by Mallory and Adams (1997: 53) at c 2600–1900 B.C., is thought to provide the first good evidence of the raising of horses outside the steppe (Bökönyi 1994). Toward the end of the millennium, there is written evidence for the arrival of domesticated horses in Mesopotamia (Clutton-Brock 1992: 89).

If we consider the horse to be a domesticated animal in PIH society, then in the first half of the fifth millennium the evidence would seem to limit the PIH area to the steppe east of the Dnieper. In the second half of the fifth millennium it spreads to the upper Balkans and the Danube valley. In the fourth millennium it spreads to include the area south of the Caucasus and into northern central Europe. In the third millennium the area is spread over much of Europe, the Near East, and the Asian Steppe.

Wool

Sheep may have been domesticated as early as 9000 B.C. (Ryder 1983: 22), but the originally domesticated sheep were used for meat only. Wild sheep are hairy, rather than woolly, although they have a wool undercoat. Woolly sheep are first indicated at Sarab in the Kermanshah Valley of Iran. A clay figurine from that area of a sheep shows clear indication of staples (characteristic clumps of wool fleece) (Bökönyi 1977: 101). There are four carbon 14 dates from that site, all calibrating to the seventh millennium B.C. The uncalibrated dates, found in Bruenig (1987: 54) are: 6006 +/- 125 B.C., 5655 +/- 96 B.C., 5694 +/- 89 B.C., c. 5900 B.C.. Moreover, Bökönyi (1977: 24) notes that 73–93 percent of the sheep bones belong to adult animals, indicating a use of the sheep for secondary products. Preservation of adults of both sexes should indicate use for wool as well as milk products. Davis (1984), however, basing his work on a 1978 excavation in the Kermanshah valley including Sarab, argued that no evidence of secondary use of sheep shows up until the beginning of the sixth millennium. Outside this area there is no indication of use of wool. Slaughter patterns for sheep and goats in eastern Anatolia and Greece do not indicate use of ovicaprids for secondary products in that area in the Neolithic (Davis 1987: 159). Significant indications of wool technology do not occur anywhere until the fourth millennium, occurring first in Mesopotamia. Wool eventually became an important item of trade in Mesopotamia. Workers wages were paid in wool, and exports of woolen cloth paid for the imports of raw materials (Ryder 1983: 97).

Although there is no evidence of woolen textiles existing before the fourth millennium, we have to entertain the possibility that wool felt may have existed earlier. Felting requires no equipment; so if the textiles themselves did not survive, we are left with no direct evidence. Ryder (1983: 735) nevertheless suggests that the first wool textiles were felt. He points out that even in wild sheep the wool undercoat molts, a process resulting in natural felting. The earliest possible attestation of felted material is, according to Barber (1991: 216–17), in Anatolia at Çatalhöyük, from 6000 B.C. This is the remains of what might be a textile made of animal hair. In a society that had developed impressive patterns on woven flax, this artifact shows no pattern. She suggests that this may be the accidental felting of hair used as matting, rather than an intentionally produced textile. It is possible but unlikely that felt textiles were produced without the presence of woolly sheep. Since many woolly sheep are required to produce enough wool for felt, it would take many more hairy sheep to produce the same amount. I will return to this issue below. For now, I will limit myself to the actually attested evidence.

In the fourth millennium, weaving of wool textiles spread from Mesopotamia to Egypt, and apparently to the Caucasus. Munchaev (1975: 161) reports a piece of cloth found at Kvacxelebi in the Kuro-Araxes culture of the southern Caucasus. He describes it as a simple weave made of wool and a plant fiber (linen). Unfortunately he does not cite a source and does not name the archaeological level. Kvacxelebi was occupied for many hundreds of years. The levels belonging to the Kuro-Araxes culture at Kvacxelebi are labeled B and C. A calibrated carbon 14 date for the top of the C layer is 3568 +/-277 B.C. (Kavtaradze 1983: 83). Kavtaradze (1983: 97) suggests that the top of the B layer should be dated to the beginning of the third millennium, a date he further argues should represent the end of the Kuro-Araxes culture in Georgia. This would seem to locate the cloth in the fourth millennium.

Other evidence seems to support this conclusion. Munchaev (1975: 161) notes the existence of pottery with impressions of cloth at Kvacxelebi and at Amiranis-Gora, from the same culture. Amiranis-Gora has calibrated carbon 14 dates of 3654 +/- 402 and 3404 +/- 416 B.C. (Kavtaradze 1983: 83). While there can be no indication of the fiber, this at least indicates the existence of some woven cloth in this area in the fourth millennium.

Furthermore, many sites in the Kuro-Araxes culture show a change in livestock breeding from an early focus on cattle to a clear emphasis on sheep. Some, as at Kvacxelebi, kept the herds of sheep near the settlement. Others, as at Amiranis-Gora, apparently practiced transhumance, moving the sheep to high pastures during the summer (Kushnareva and Chubinishvili 1970: 109; Kushnareva 1993: 223–24). Kushnareva and Chubinishvili see as evidence of transhumance the existence of large buildings for the winter housing of sheep, as well as enclosures for stock in high pastures.

Kushnareva and Chubinishvili identify the sheep at Kvacxelebi as of the "imeretinskii" type. These were presumably what Ryder (1983: 268) calls "imeritian," a Caucasian fat-tailed breed used for milk products, meat, and wool. Ryder notes that a similar breed, the Karachaev (which he observed), produces a coarse wool. These sheep were kept outside the year round, as the sheep at Kvacxelebi apparently were.

Remains of churns and dishes appropriate for cheese production are found at Kvacxelebi. It may well be that milk was more important than wool for settlements of this type, but the cloth indicates that wool was used. Although there are no indications of looms, spindle whorls are found throughout the Kuro-Araxes culture. In kurgans of the Bedeni culture, which follows the Kuro-Araxes culture, felt and woven wool cloth are found in burial chambers (Kushnareva 1997: 213). Barber cites the discovery of a twill cloth (presumably wool) in a burial at Martkopi, which has a similar early third millennium date.

In the northern Caucasus, Barber (1991: 168–69) reports a kurgan burial at Novosvobodnaya in which were found the remains of clothing on a male skeleton. There was an undergarment of a linen-like fabric and an outer garment of a fluffy (puxovyi) yellow and black plaid. Since this cloth is contrasted to linen, we might assume that it was wool. Novosvobodnaya is either a late version of the Maikop culture or a new culture that replaced it. I mentioned above in the discussion of horses that a general range for the Maikop culture (including Novosvobodnaya) would be c. 3750–2750. Barber (1991: 169–70) also notes a burial of similar age in the steppe along the lower Volga that has the trace pattern of a rug used in the burial.

Bökönyi (1987) argues that wool sheep were introduced into Europe via the Pontic steppe in what he calls the third millennium. He explicitly says that he is not using calibrated dates yet says that the introduction of sheep from the steppe into eastern and northern Europe is coordinated with Gimbutas's second and third kurgan expansions. This would mean that wool sheep had reached the western edge of the steppe by c. 3400–3200 B.C. This agrees with Sherratt (1983b), who says that wool sheep are found in the Pontic steppe, the Balkans, and eastern Europe in the fourth millennium and that the steppe route is the most likely for the introduction of wool sheep. He also says that the Corded Ware culture (dated by Mallory and Adams [1997: 127] as beginning about 3200 B.C.) was the vehicle by which wool sheep spread throughout northern Europe.

The best evidence for the introduction of wool sheep into central Europe comes from the influx of kurgan burials in the Danube valley, beginning in the second half of the fourth millennium. Bökönyi (1974: 168) identifies markings on clay statuettes of sheep found in Hungary from this time as representing fleece. Ecsedy (1979: 15, 16, 19, 20, 37–39, 44) mentions the remains of blankets, spreads, and carpets made of organic material in early kurgan burials in the Carpathian basin and elsewhere. In general, the material itself does not survive, only the pattern in the earth. Ecsedy cites no carbon dates from any of the kurgans, but he does say that the early kurgan burials are contemporary with the Usatovo culture, which Gimbutas (1997: 55) dates from the second half of the fourth millennium. Unfortunately, the only surviving textile that was analyzed (a piece of a tent from an undated kurgan burial at Gurbaneşti) was analyzed as camel hair (Ecsedy 1979: 39). The new sheep that moved from the steppe into Hungary had an average height at the withers of 70 cm. compared to 60 cm. for Eneolithic sheep (Bökönyi 1971). The number of sheep increased dramatically, and a larger number of these sheep were allowed to live into adulthood, indicating use for secondary products. Tsalkin (1972) found that by the late Bronze Age, the mean height of sheep throughout eastern Europe was 70 cm.

In the Trichterbecherkultur (TRB, German for Funnel-necked Beaker Culture) of north-central Europe, the first indications that sheep are being raised for anything other than meat is very late, near the end of the fourth millennium. At Spodsbjerg on Langeland more than half the ovicaprids found had lived past adulthood, and most had died after their sixth year. The data from Schalkenberg in the Bernburg culture indicate that about 20 percent lived past four years (Midgley 1992: 382, 384). This is the earliest indication of use of sheep for secondary products, but it does not differentiate between use for milk and use for wool. The Bernburg culture is dated by Midgley (1992: 227) from 3200/3100–2900 B.C. This is certainly consistent with Bökönyi's timetable for the introduction of wool sheep into this area. Sherratt (1983b) associates wool with the Corded Ware culture (normally considered to contain IE speakers as a major component) and suggests that it is reasonable to think that wool was introduced into northern Europe by this culture. Interestingly, Bökönyi (1987) finds evidence for the spread of Caucasian sheep along the southern coast of the Black Sea at the same time that they were spreading across the Pontic steppe.

The main problem tracing the origin of this new variety of sheep is not their movement into Europe but how and when they arrived in the steppe. We have noted evidence for wool technology in the southern and northern Caucasus in the fourth millennium. Certainly sheep domestication existed in the Caucasus in the fifth and sixth millennia, but there is no direct indication that they were used for or were useful for wool. Sheep were there but not as an important feature in the Maikop culture of the northern Caucasus. The primary focus of their livestock economy was swine (Munchaev 1975: 382). In population, sheep were in third place, after cattle. The Maikop culture never occupied the high altitudes best suited for sheep raising. This does not necessarily mean that the people of the Maikop culture did not know about wool, but it makes it less likely that they produced much. It also means that the Maikop culture is not a good candidate for the vehicle whereby wool sheep were introduced into the steppe. If wool sheep were introduced into the steppe through the Caucasus, they probably came from the northeastern area, where settlements of a culture similar to the Kuro-Araxes culture reached the steppe. In fact, sheep-based livestock economy seems to have developed in the northeastern Caucasus earlier than in the southern Caucasus. Gadzhiev (1991: 82) describes the Eneolithic settlements at Ginchi and Chinna as permanent agricultural settlements with primarily sheep as livestock, using high alpine pastures and transhumance. Temporary sites in the high altitudes confirm this.

Shnirelman (1992) suggests that the tall sheep that moved from the steppe into central and northern Europe had been in the steppe since the Eneolithic, and that these sheep were introduced from the Caucasus c. 5800–4800 B.C. The Eneolithic sheep in the Caucasus were similar in size to the steppe variety. His primary evidence comes from Petrenko (1984). However, Petrenko (1984: 49) preferred the area of Turkmenia near the southeast corner of the Caspian as a source. This is near the area where the earliest evidence of domestication is found, and the Eneolithic sheep from that area are also tall.

It is certainly worth examining Petrenko's data in detail. The earliest sheep bones found in the steppe that are intact enough to allow measurement are found at the Khvalynsk cemetery in the forest steppe zone along the lower Volga. This is the culture Gimbutas claims as the parent culture of the Proto-Indo-Hittites (1997: 56). With the Sredny Stok culture in the west, the Khvalynsk culture formed the basis of the Yamnaya or pit-grave culture of the steppe. We have six carbon 14 dates from skeletons in the cemetery. Three, recently published in Anthony (1998), date it to the late sixth to early fifth millennium:

Grave 30: 6200 +/- 85 BP, 5251–5010 B.C.
Grave 18: 5985 +/- 85 BP, 4946–4783 B.C.
Grave 18: 6015 +/- 85 BP, 4994–4799 B.C.

Two of the three dates from Agapov et al. (1990: 83) roughly agree:

Grave 4: 5903 +/- 72 BP, 4863 +/- 137 B.C.
Grave 26: 5808+/- 79 BP, 4768 +/- 136 B.C.

Grave 13, however (in the Agapov study), had a date of 4026 +/- 57 BP, which would calibrate to the middle of the third millennium, if correct. If the sample itself is not contaminated, this anomalous date likely represents a later intrusive burial. It is probably safe to stick to the earlier dates and to date the animal remains at the site to the early fifth millennium.

Petrenko found measurable bones in the sacrificial areas—(zhertvenniki) nos. 1, 10, and 11—and in burials 101 and 115. In zhertvennik 1 he found the remains of one sheep that would have been approximately 73 cm tall at the withers. In zhertvennik 10 he found the remains of at least fourteen sheep whose range of height would have been 61.2–72.6 cm, averaging 65.77 cm. In zhertvennik 11 he found the remains of four adult sheep that would have been about 78 cm tall. In burial 115 he found the remains of twenty-two sheep with approximately the same range and average as those from zhertvennik 10. He does not give measurements from burial 101.

This seems to provide good evidence for the existence of tall sheep in the steppe in the fifth millennium, but it says nothing about any earlier period, and the data leave me with some questions. Clearly, Petrenko has found at least five tall individual sheep. There are thirty-six sheep in two groups of fourteen and twenty-two whose average height (c 66 cm) is closer to the 62.5 cm he cites as the average height of east European Eneolithic sheep than to the height of the tall sheep. It would be very useful to have radio carbon dates for the bones of the tall sheep. If the tall individuals belong to a later period, then we could be documenting the arrival of tall sheep during the Khvalynsk culture, rather than demonstrating that domesticated sheep on the steppe had always been of the tall variety.

Nevertheless, it is likely that the tall variety of sheep introduced from the steppe into central Europe existed in the steppe in the early fifth millennium. It is also reasonable to speculate that these sheep produced usable wool in the fifth millennium and must surely have produced usable wool by the second half of the fourth millennium. Because sheep can be tall and hairy and, therefore, producing no or little usable wool, and because we have no evidence of woven cloth or looms, we do not have unambiguous evidence one way or the other for the existence of wool-producing sheep in the steppe before the fifty millennium.

This brings us to the question of the use of felt, which would have left little or no archaeological trace. Before discussing that, however, I would like to look seriously at the PIH word for *wool.*

One could always wonder if the word for wool predates the use of wool as a product. It might conceivably have meant *hair of a sheep.* The stem for *wool,*

**HwlHn-*, if not a borrowed word, is much too long not to have been derived. We might well find candidates for a root or stem from which it derived.

Gamkrelidze and Ivanov (1995: 498–99) relate **HwlHn-* to a basic root for weave *H*(*e*)*w-* found in the Sanskrit, infinitive *ótum* < **au-tum* <**eu-tum* < **Hew-tu-m; otu-* (woof), *ūta-/uta* (pp. woven). The present is *váyati* < **Hwejeti*, meaning in Sanskrit *weave, interweave, braid, plait*. The simple stem **H*(*e*)*w-* is found only in Sanskrit. Baltic and Slavic have a verb with a present tense apparently cognate with Sanskrit *váyati:* Lith. *výti, veja,* Rus. *vit', v'ju.* Both mean *twist* and are used to refer to the making of nests. In other languages a variety of forms may begin with **Hew-*, or *Hwe-* and mean *weave*. One should remember that the laryngeal rarely shows up at all, and never before *w* plus a vowel, so there is multiple ambiguity. A more frequent word for *weave* is **w*(*e*)*bh-*, presumably **Hwebh-* with a *-bh-* "stem-extension": Sanskrit *ubhnāti* (ties), Greek *huphaínō* (I weave), Old English *wefan*, Tocharian B *wāp-*, Osetian *wafyn*, Persian *baf-* (weave), Avestan *ub-daēna-* (from cloth). A form with *-dh* is found in Old Norse *váđ* (spinning), Gothic *gawidan* (tie), Old English *wǣd* (clothing), Armenian *z-audem* (tie). Lithuanian *áudžia* (weaves) could have come from **Howd-* or *Howdh-*, as could the base of Russian *usló* < **oud-tlo,* (starting cloth on a loom). English *wattle* may be related to the same root.

One may or may not believe that all these words come ultimately from a single root. If they did, the meaning of the root was probably much more primitive than *weave* in the technical sense of *make cloth on a loom.*

What we would like to find is a related word which simply lacks the final *-n-*, which is a common suffix in Indo-Hittite. Practically speaking this means that we look for roots listed in Pokorny (1994) as *w*(*e*)*l*(*ə*)*-*, and then look for evidence of laryngeals where they might show up. Pokorny (1994: 1,139) suggests that the word for wool might be related to roots **wel-* which mean either *pluck* or *roll.* Meaning *pluck*, Latin *vellere* is actually used to refer to the plucking of sheep, which is the ancient method of obtaining wool. The double *ll* could come from a variety of clusters (**ld, ln, ls*). Pokorny, without citing any evidence, traces it to an original *-ls-* cluster.

Several words meaning *hair* or even *wool* are formed from the root **Hw*(*e*)*l-*. Hittite has another word for *wool, huliya-*, which has a *-j-* suffix. Lithuanian *vãlas* (hair from a horsetail, fishing line made from horsehair) and Sanskrit *vālas,* (hair from an animal's, esp. a horse's, tail) both may have come from *(*H*)*wol-o-s.* The length in Sanskrit is explained by Brugmann's law, which determines the lengthening of original **o* in open syllables. Slavic **wols-o-s* (OCS *vlasŭ*, R. *vólos*) (hair), is cognate with Avestan *varəsas* (hair). Neither of these words should have had a laryngeal following the **l.* A laryngeal would block the lengthening of the **o* in Sanskrit *vālas* and should have conditioned acute intonation in Slavic **wolsos,* giving R. **volós* rather than actual *vólos.* Greek *láknē* (woolly hair, down, sheep's wool) and *lákhnos* (wool) likewise lack evidence for a laryngeal. Pokorny (1,139) traces the Greek words to **wl̥ksn-*. Latin *vellus,* [gen.] *velleris* < **wellos-* (fleece) is probably a derivative from *vellere.*

Under the general rubric of Pokorny's *_wel-_ (roll), there are Germanic, Baltic, and Slavic verbs meaning _felt_ or _full_ that indicate processes applying only to animal fiber, primarily to wool. Lithuanian _vél-ti_ and Latvian _vêlt_ mean _full, felt, beat, roll, rumple, throw down, card_ (wool). Russian _valjat',_ which is a transparent iterative from the same root as Lithuanian _vélti,_ means _roll, full, felt, knead_ (dough), _drag,_ and from the same root _valit'_ can mean _throw down_ or _flock together._ German _walken_ (full, felt, pummel) is cognate with English _walk._ In Old English _wealcan_ meant _toss around, roll, move around,_ but _wealcere_ meant _fuller._ Barber (1991: 216) notes that in Scotland a wa(u)lking song is sung while trampling newly woven tartan cloth, soaking wet, for felting.

This array of meanings seems bewildering, but it makes more sense when we read Barber's (1991: 216) description of the process of felting: "To make fiber felt, the folk of Asia and India lay out their cleaned wool on a mat in layers corresponding in area and shape to what is desired for the finished piece. Warm water or whey is sprinkled on, and the mat is rolled and rerolled, kneaded, beaten, stamped on, dragged about, and otherwise subjected to as much friction and pressure as possible over many hours, until the fibers are thoroughly interlocked." Ryder (1983: 265) even notes that the wool may be combed before felting. Fulling is the same felting process applied to woven wool, except that it does not need rolling.

Greek has what were or had become two verbs with a present tense _eileō_ < *_welneō._ The verb meaning _press, squeeze, force together_ has an aorist _elsa_ < *_welsm,_ perfect _eola_ < *_wewola._ These clearly show that the stem was *_wel-_ and therefore the present has an _-n-_ suffix. A second verb _eileō_ (_turn, roll_) is a regular verb with aorist based on the present stem. Chantraine (1968) and Frisk (1960–70) relate the first verb to Lithuanian _vélti_ in the meaning _felt_ and the second to _vélti_ in the meaning _turn._

Armenian has a noun _gelmn_ (wool, fleece), which Fraenkel (1962–64: 1254) glosses as _felt_ (_filz_) as well. Mann (1963: 8) relates this form to Lithuanian _vélti._ In this form the presence of a laryngeal after the _l_ is nondistinctive. Mann gives *_welmn_ as the Proto-Armenian form, while Beekes (1988) treats it as possibly from PIE *_HwelHmen-._

Of all the examples, the Lithuanian verb _vél-ti_ is the clearest formal match. The Lithuanian word for wool is _vìlna._ Despite the graphic difference in the accent marks, the two words agree in intonation—both have acute intonation. For primary words, this normally means that there was a laryngeal following the _l._ In pre–Balto-Slavic terms, then, the verb stem had the shape *_welH-,_ while the word for wool was *_wlH-n-eH_$_a$ (Any initial laryngeal would have disappeared earlier.) The only difference in the basic stem is in the ablaut grade of the root, which often changes in derivation. Given the similarities in form and the meaning of the verb, it is surprising that none of the standard etymological dictionaries even mention a direct connection between _vélti_ and _vìlna._ Pokorny (1994: 1,139), as I noted above, simply mentions the possibility that the word for _wool_ is related to the roots for _roll_ or _pluck._

Under the item *vélti,* Fraenkel mentions words for wool as "ferner" relatives, but does not mention the possibility under *vìlna.*

The indication of an ancient connection between the word for *wool* and the process of felting raises an obvious question. How old is felting? The answer, unfortunately, is that both in general and for the area of Eurasia that most interests us, we do not have the faintest idea. Barber, in summarizing her views on the history of textiles (1991: 250), divides western Eurasia and the Near East into four areas. Her southeast and southwest areas center in Mesopotamia and Egypt, respectively. Those cultures had weaving very early and used ground looms. Of immediate interest to us are her northwest and northeast zones.

The northwest zone includes Anatolia, the Balkans, Italy, the Danube Valley, and areas north and west of that. This area could be identified with the farming communities that moved from Anatolia to the Balkans and spread north and west from there. In this area we find flax from the very beginnings of farming, with wool introduced much later. The weavers presumably used band looms (small looms with tension supplied by a band around the weaver's back) for making narrow strips of cloth and what she calls warp-weighted looms for making larger textiles. Even where textiles were rarely preserved, weights serve as probable indicators of weaving. Significantly, none were found before the second millennium B.C. north of southern Poland, and none were found in the Russian steppe.

The northeast zone is an area that used primarily wool. Barber postulates that large cloth was made by felting but that small band looms were used for making narrow strips of cloth. For early periods, however, these assumptions must remain only assumptions. Except for the area of the northern Caucasus, there is no evidence from the early periods. Barber assumes that this area produced felt because there are no traces of large looms, and from the time people of this area appear in history, they produce felt. The problem is that they do not appear until the first millennium B.C. In northern Europe, evidence of felting or fulling occurs as soon as wool is found.(Barber 1991: 217). She dates this as late Neolithic or early Bronze Age.

The earliest possible attestation of felted material is, according to Barber, in Anatolia at Çatalhöyük, from 6000 B.C. As noted above, she suggests that this may be the accidental felting of hair used as matting rather than an intentionally produced textile. According to Barber, the first firmly attested felt is found in Anatolia in the third millennium. Barber suggests that this is in a culture transplanted from the steppe (251). Even if this is true, these Anatolians had probably lived in the upper Balkans for a long time before crossing to Anatolia, so they had plenty of contact with weighted warp looms. More intensive evidence of fiber felt is found in Gordion, the Phrygian capital, in c. 700 B.C. The Phrygians were Indo-Europeans who likewise moved from the Balkans into Anatolia, but it is a stretch to call them steppe people at the time of the move.

Barber identifies the Proto-Indo-Europeans as inhabitants of the northeast felting zone (1991: 254–55). Significantly, however, she leaves the area of the Caucasus as a large question mark. We have seen evidence of early woven wool from the southern Caucasus and reference to later burials containing felt coverings.

To return to our etymology of **HwlHn-*, we can say that there was a family of words built on an initial **Hw(e/o)l-* meaning *hair* or *wool*. Various suffixes, such as *-s-*, *-g-*, *-j-*, *-H-*, either form specific nouns or form verbs indicating processes applied to animal hair or wool. The stem **HwlHn-* is a tertiary derivative, indicating a particular type of animal hair, wool, which is appropriate for processing or has been processed into a textile. It is this word for wool that has the best PIH etymology, and if this explanation of its origin is approximately correct, then it is firmly tied to the secondary products revolution.

Terminus Post Quem

We are now in a position to put at least a boundary on the time of PIH unity. It has to have been a time when ox traction, horses, and woolly sheep coexisted. If we limit ourselves to hard evidence, then we would place the earliest possible time in the fourth millennium B.C. The Russian steppe had horses and putative woolly sheep in the fifth millennium but no direct evidence of ox traction until the fourth. If Shnirelman (1992) is correct in his claim that the tall sheep were introduced to the steppe from the Caucasus, then the southern Caucasus also possibly had woolly sheep and horses in the fifth millennium. Again, no direct evidence of ox traction appears before the fourth millennium. Mesopotamia did not have horses until the third millennium, and there is no evidence of horses or woolly sheep in north-western Anatolia until the fourth millennium. Northern and central Europe have no evidence of wool or ox traction before the fourth millennium.

To be safe, we could propose a *terminus post quem* for the division of PIH in the late fifth millennium B.C. At this point we can say nothing definite about the place where the first division took place. It is as least interesting, however, that the very earliest places with all three cultural items are found in the vicinity of the Black Sea, the best candidates being the Pontic-Caspian steppe and the area south of the Caucasus.

One thing we can say is that this date eliminates the proposals by Renfrew and Drews (1997). At the time of the migration of farmers from northwest Anatolia to Greece, there was no ox traction, no wool technology, and no trace of horses on either side of the Bosporus. Even if the recent reports of horses found in south-central Anatolia prove correct, that should not matter. The Balkans have been well studied, and no horses show up until the fifth millennium. The seventh millennium farmers who migrated to Europe would have lost any word for *horse* from their language.

Proto-Indo-European and the Wheel

There is a complex of reconstructed words that are PIE, but not demonstrably PIH, and that deal with wheels and wheeled vehicles:

> *_k^wek^wlos_ (wheel): OE _hvéol_ (wheel, circle), OHG _wēl,_ ONorse _hvél_ (wheel), Gk. _kuklos_ (wheel, circle), Skt. _cakras_ (wheel, circle), Av. _čaxras_ (wheel) Tocharian A: _kukäl_ (wagon).

This is a clearly reconstructible PIE word, not just a root. It is a reduplicated noun, which is unproductive in all the language families—not that unusual in the oldest vocabulary. It is from the verbal root *_k^wel-_. Its attested verbal forms mean _move_ (Skt. _cárati,_ Gk. _pélomai_) or _turn_ (Av. _čaraiti,_ Albanian _siéll,_ Cuneiform Luvian _ku-wa-li-i-ti_) (Melchert, 1993, 114). One might wonder whether the original meaning of the noun were _wheel_ or _circle,_ but the meaning _wheel_ is firmly PIE.

One possible problem for the unity in the meaning is East Baltic (Lith. _kaklas,_ Latv. _kakls_ [neck]). This technically does not match *_k^wek^wlos,_ since the first vowel is the reflex of *_o,_ not *_e._ However, there are enough examples of unexpected _a_ from etymological *_e_ in east Baltic to make one careful. Latin _poples, poplitis_ (knee joint, back of knee), if borrowed from an Italic language with _p_ for *_k^w,_ could come from *_k^wok^wl-et-_ (Mallory and Adams 1997: 640). This enhances the possibility of a term for rotating joint with o-grade. If these words are cognate, they fit a general meaning of _something which turns,_ they but do not mean _wheel._

Other examples of forms from the same root meaning _wheel_ or _things that turn_ are: Sl. _kolo(*s),_ OPr. _kelan_ (wheel), Latv. _ducele_ (two-wheeled cart), Gr. _polos_ (axis < *_k^wolos),_ L. _colus_ (distaff).

> *_rot(e)H_a_: L. _rota,_ Germ. _rad,_ Lith. _ratas,_ Irish _rath_ (wheel); Skt. _rathas,_ Av. _raθa-_ (cart, chariot).

These words are derived from a verbal stem attested in Old Irish _rethim_ (I run). The Balto-Slavic verb represented by Lithuanian _rita_ (rolls), Rus. _rtut'_ (an old participle < _*rĭt-ontĭ_ [mercury]) is almost surely cognate. The short _ĭ_ in Baltic and Slavic is a regular morphophonemic realization of zero-grade ablaut in Balto-Slavic.[3] There is an apparent phonological problem with the _-th-_ in Sanskrit, _θ_ in Avestan. This indicates a cluster with a laryngeal. Since this cluster has no effect in the other languages, we can simply assume that the verbal stem was *_r(e)tH-._

In this case we may doubt whether the nouns represent an original PIE stem. Deverbal derivatives with o-grade ablaut remained productive for millennia in the Indo-European languages, and these words, therefore, could have been formed in the various languages at any time. However, the nouns are supported by better evidence and are more consistent in meaning than the verb. They should be reasonably old. If we wish to make this a PIE stem, we should consider it an adjectival derivative, with gender varying according to the gender of the modified noun. This can account for the fact that the resultant noun is masculine in Indo-Iranian, feminine in Latin, and neuter in Germanic. Substantivization of adjectives is a normal process in Indo-European languages. For the verbal meaning, we might as well believe the meaning of the Balto-Slavic verb, since the Irish is of no help. The adjective **rotH-os, -om -ā* could then mean *rolling*. This adjective could apply to either the wheel or to the vehicle to which the wheel is attached.

> *H_a*aḱs-* (axle): ON *öxull,* L. *axis,* Gr. *áksōn,* OCS *osĭ,* Lith. *ašis,* Skt. *akṣas,* Av. *aša-* (axle), Irish *aiss* (cart)
>
> **nobh-* (naval, hub): In Skt. *nābhi-,* OP *nabis,* and Germanic (OHG *naba,* OE *nafu,* ON *nǫf*) the word for naval is used for the hub of a wheel. This may be accidental convergence in meaning in Germanic and Sanskrit. Old Prussian was subject to strong influence from German.
>
> **weǵhe-* (transport by vehicle): OCS *vezǫ,* Lith, *vežu,* Skt. *váhati,* Av. *vaza-iti,* Latin *vehō* (transport), OE *wegan* (carry). The o-grade derivatives from this verb typically mean wheeled vehicles: OCS *vozŭ* (wagon), ON *wagn,* Gk. w*ókhos* (cart), Mycenaean Greek *wo-ka* (chariot). This is only one of the meanings of this verb. Although the o-grade derivatives support the claim that this term was used for transport with wheeled vehicles before the breakup of PIE, it is clear that the vehicles in question did not have to have wheels. In Lithuanian, Russian, Old Church Slavic, Latin, and Sanskrit, the verb could apply to transport in ships, and in Lithuanian and Russian it can apply to sleighs. Latin *vehiculum* and Sanskrit *vahitram*, which may be cognate (*w*egh́Htlom*) or may be independent derivatives with a productive suffix, both mean either wheeled vehicle or boat.[4]

None of these words have cognates in Anatolian languages. Unfortunately we do not know the words for *axle* or *hub* in Anatolian. The only specific disagreement is in the word for *wheel*. The Hittite word is *hurki,* which can be a derivative from a verbal stem **Hw(e)rg-* found in Sanskrit *várjati* (twist, turn). Tocharian A *wärkänt* (wheel, circle) may be formed from the same verbal stem but is not at all the same word. The word for *wagon* in Hittite is *tiyarit-,* and *huluganni-* is some other kind of vehicle.

Despite the fact that we can never be sure that the absence of this set of cognates in Anatolian is significant, it should be useful to try to find out whether it makes sense to talk of a split between Proto-Indo-European and Proto-Anatolian after the time of the introduction of the horse, wool sheep, and the yoke, but before the introduction of the wheel.

The date and place of the invention of the wheel is open to question, assuming there was only one event of this nature. Piggott (1991: 18) dates the appearance of signs for wheeled vehicles in Mesopotamia to the late fourth millennium. Actual wheels are not found there before the third millennium, a fact that must be considered alongside the fact that in the rest of the world we do not have a written record.

All we have are the first attestations of wheels or models of putative wheels in archaeological finds. Actual wheels are found in various places in Europe in the second half of the fourth millennium. An axle and wheels unit found in Switzerland dates from c. 3400 B.C. (Piggott 1991: 18), Sherratt 1996). A clay model of a wagon with four wheels found in the Baden culture in Hungary dates from between 3400 and 2900 B.C. (Gimbutas 1997: 255), and a burial with a two-wheeled cart found in the steppe tentatively dates from the fourth millennium (Gimbutas 1997: 83, 187). In the TRB culture, representations of apparent wheeled vehicles on pots from Bronocice and Ostrowiec Święţokrzyski are likewise dated to the last half of the fourth millennium (Midgeley 1992: 378–79).

In the area north of the Caucasus, a wagon burial is found in the Novosvobodnaya (late Maikop) culture (Kondrashov and Rezepkin 1988, Chernykh 1992: 69). Numerous wagon burials are found on the steppe north of the Caucasus in kurgans of the Novotitorovka type (Chernykh 1992: 79, Gei 1991). I noted above, in the discussion of the horse, that the approximate dates of the Maikop culture were 3750–2750 B.C. There is no radio carbon date associated with a wheel or wagon found in the Maikop culture. Two carbon 14 dates have been assigned to wagons found in burials from the Novo-Titorovka culture immediately north of the Maikop culture. One has a carbon 14 date of 4440 +/- 40 BP, calibrated to c. 3100 B.C. (Gei 1991). Gei points out that this burial is typologically younger than other wagon burials in the same culture. However, a typologically older burial has a date of 4270 +/- 40 BP, c. 2900 B.C. One of the two sites must have had some contamination, and since the younger date was found closer to the surface, he thinks that that date is most suspect. Overall, we have no evidence to place the appearance of the wheel in the steppe or the northern Caucasus before the beginning of the second half of the fourth millennium, but it almost surely did occur before the end of the fourth.

In the southern Caucasus, burials with wagons have been found in kurgans at Bedeni, Trialeti, and Zurtaketi (Masson 1973). As of Kushnareva and Chubinishvili (1970: 111) a wagon found at Bedeni was the earliest known wheeled vehicle from the southern Caucasus. Chernykh (1992: 98–102) locates the Bedeni culture in the middle Bronze Age, beginning in the period 2700–2500 B.C. The kurgans on the Bedeni plateau are dated by Kavtaradze (1983: 108–9) in the second quarter of the third millennium B.C. Kavtaradze used calibrated carbon dating, but the actual kurgans for which dates are available are in the Alazan valley, considered contemporaneous with the Bedeni culture. Dating from about the same time are models of wheeled vehicles found at Sharak (Khachatrian 1975: 74, 77). The Trialeti kurgans with wagons are later, dated by Kavtaradze (1983: 117–8) in the second half of the third millennium. The Zurtaketi burials are considered to be part of the Trialeti culture.

Because disc-shaped clay objects with holes in their middles have been found in a variety of cultures, some archaeologists, seeing model wheels among these objects, push the origin of the wheel much more deeply into the past. Kushnareva (1997:

170) asserts that model wheels "have been found at many agricultural settlements in southwestern Asia dating to the sixth to fourth millennium B.C." Dinu (1981) identifies model wheels from the Cucuteni A and Karanovo VI cultures west of the steppe (Gimbutas 1985, 1997: 308–9; Dinu 1981). These cultures date to the mid or late fifth millennium. Munchaev (1975: 200) claims to identify model wheels in the southern Caucasus site of Tekhut, dated in the fifth millennium. These claims depend on the ability to distinguish model wheels from spindle whorls. No model wheels are found attached to model carts in the southern Caucasus until the third millennium, at roughly the same time that actual wheels are found. Dinu's model wheels are at least large, up to 11 cm. in diameter. Jeanine Davis-Kimball, however, in a personal communication, suggested that even those wheels are not too big to be spindle whorls used for bast fibers. Gusev (1998) also discusses "model wheels" found in the Cucuteni-Tripol'e culture but offers a different argument. He argues that some discs are too light to serve as spindle whorls. The discs he studied are 1–6 cm. in diameter, 0.3–0.7 cm. thick. He suggests that they were used as wheels for vessels in the shape of oxen, found with legs and holes in their feet that could hold wooden axles for the wheels. Earlier zoomorphic vessels without these holes exist. These are found in the general area where the ceramic models of sledges are found (western Tripol'e) and at either about the same time or slightly earlier (c. 4000 B.C.). Nevertheless, Gusev himself notes that zoomorphic figures with wheels on their feet do not guarantee the existence of wheeled vehicles; wheeled animal figures are found among the Aztecs, who never used wheeled vehicles (Piggott 1983: 15).

We can now look at the effect of using the appearance of wheeled vehicles as a *terminus ante quem* for the division of PIH into PIE and PA. Candidate PIH cultures must therefore show evidence of having had wool, horses, and ox traction before the introduction of the wheel. In the Khvalynsk-Yamnaya complex in the Russian steppe, evidence of wool and horses clearly precedes evidence for wheels. Since there is early evidence for sledges, it seems reasonable to assume that ox traction with sledges preceded wheeled vehicles. In the southern Caucasus, we must ignore the arguments for model wheels as evidence for wheeled vehicles. If we do that, then a thousand-year cushion separates the earliest evidence for yokes, wool, and horses from the earliest attested wheels. Curiously, no early evidence of wheels has been found in the area of the western end of the Black Sea, around the Bosporus. This could be accidental; some of the earliest evidence for wheels is found in the Danube valley. However, a mountain range separates this the area of the Bosporus from the Baden culture, which has the early evidence for wheels. The Sherratt and Sherratt (1988) proposal then remains viable.

Not only are these areas viable possibilities, they seem to be among the best possible candidates in the ancient world. Mesopotamia proper had the wheel before the appearance of horses. When we look at central and northern Europe, we find it difficult to argue that wool technology appeared before wheeled vehicles. In the Asian steppe, Indo-European–like cultures appear after the appearance of such cultures west of the Urals, and there are reasonable arguments for an expansion of the steppe cultures east from the Russian steppe. In this view, the movement involved initial population of an area that had previously been essentially unusable for human habitation. Little doubt exists, therefore, that the population would have brought its language with it.

Despite the general viability of the area south of the Caucasus, the specifics of the Gamkrelidze-Ivanov proposal do not fit this hypothesis. Not only do they treat the word for wheel as PIH; they have the dispersal of the PIE branch of the family from the southern Caucasus. They treat the Yamnaya culture as Indo-European, but not Proto-Indo-European. It is, according to them, the homeland of the north-west European branch of PIE (Balto-Slavic, Germanic, Celtic, Italic). Since the Yamnaya culture existed for several centuries before evidence for the appearance of wheeled vehicles in the steppe, this hypothesis runs into trouble. It becomes very difficult to explain why the Indic languages, which spread directly from the southern Caucasus, have the same word for wheel as NW PIE languages, which apparently did not get the wheel until they got to the steppe. There may be ways to salvage a southern Caucasus homeland, but I do not have time to discuss that in this forum.[5] I will spend the rest of my time contrasting the Sherratt and Sherratt proposal with the Mallory-Gimbutas proposal.

The Sherratt and Sherratt Proposal

Like Renfrew, the Sherratts (1988) attribute the origin and initial spread of the ancestors of the Indo-Europeans to the development and spread of farming from the Near East. They, however, attribute the initial stages, which involves the spread of the ancestral language into western Anatolia and the Balkans, to what they call preProto-Indo-European. Proto-Indo-European itself, according to them, was formed by convergence in the trading network around the western end of the Black Sea:

> One of the phenomena noted by linguists in areas such as the Caribbean or Melanesia is the formation of pidgins and creoles in connection with the growth of trading networks, especially those based on coastal contacts. If we postulate a coastal koiné around the shores of the Black Sea (and perhaps also the Aegean . . .) in the 3rd millennium, bringing together elements from **PPIE languages already existing in Anatolia and perhaps in the Balkans and western Pontic area, these requirements are fulfilled. After all, boats–both large and small—are attested in the protolexicon as well as horses and carts, and the wealth of Troy II indicates the potential scale of trading at this time.

They admit that population movement might have played a role in the spread of the language. The examples they cite are the movement of the Pit-Grave

(Yamnaya) groups from the Pontic steppe to dry areas of Romania and Hungary, and the movement of the Yamnaya people into the essentially uninhabited areas of the Asian steppe. They consider trade to be a possibly more fundamental factor. This involves a change in the Bronze Age from exchange networks to directional trade. They also cite the emergence of new metallurgy and the switch to pastoral economies, with the possibility that one language group might have controlled essential resources like wool and metals.

Certainly, problems emerge in the Sherratts' view of the spread of language. Their mention of pidgins and creoles is unfortunate and unnecessary. Zvelebil (1995) also suggests that PIE was spread by creolization and pidginization. This betrays a lack of understanding of pidgins and creoles. Pidgins typically arise when a native population tries to communicate with an elite, generally intrusive, group that has no interest in learning the language of the local population. Pidgins have vocabulary, mostly that of the elite group, but essentially no inflectional grammar. Creoles may arise from pidgins, when the pidgin is adopted as the language of a group of lower status. As the pidgin becomes a true language, its complexity increases. Nevertheless, this kind of creole gathers most of its vocabulary from the elite language and manifests severely restricted inflectional grammar. Not everyone believes that creoles must go through a stage of pidginization, however. Thomason and Kaufman (1988: 48) suggest that it is enough that the language of the dominant group is so distorted by the non–native-speaking population that it is no longer recognizable as genetically related to the source language. Anyone who knows anything about an older Indo-Hittite language knows that it does not lack inflection. Nor is it true that we cannot demonstrate genetic affiliation of Indo-Hittite languages. The standard of reconstruction to which creoles are compared is the reconstruction of Indo-European. With the Thomason and Kaufman definition, if we said that Indo-Hittite languages spread as creoles, we would be saying that the reconstruction of Indo-Hittite languages does not meet the standard of reconstruction set by Indo-Hittite languages.

There are also some possible chronological problems with the Sherratts' proposal. If I interpret the evidence correctly, other articles by Andrew Sherratt fail to support the arguments of Sherratt and Sherratt (1988).

First of all, they locate the formation of the koiné in a trading network around the western end of the Black Sea in the third millennium. This is the time period of the development Mallory (1989: 239) calls the circum-Pontic interaction sphere, which he says was established around 3000 B.C. Mallory, Gimbutas, Merpert, and other scholars associate this interaction primarily with intrusion from the Russian steppe, not from influence from Anatolia. For my purposes here, it is most important that it is a third millennium phenomenon.

This chronology would mean that PIH speakers played no role in the formation of the Corded Ware culture, since that culture has its roots in the fourth millennium in northern Europe, well away from the western end of the Black Sea.

There would likewise be no possibility that PIH speakers formed part of the Yamnaya or Pit-Grave culture of the steppe, which begins even earlier. This is important for a variety of reasons.

Sherratt and Sherratt mention two instances of population movement connected with the spread of PIH. One is the populating of the nearly unpopulated Asian steppe from the Russian steppe by people of the Yamnaya culture. The other is the movement of Yamnaya people up the Danube valley. This movement is discussed in detail in Sherratt (1983a: 37–38). He provides the best evidence of which I am aware for population movement into a populated area of Europe. He notes a drastic reduction in the number of populated sites in the early fourth millennium, which he interprets as due to salinization of the soil. Then, in the period 3500–2900 B.C., burial mounds of the steppe type occur in the Hungarian plain. The material culture and burial rite represented by these tumuli differ sharply from the local Baden culture. Moreover, the tumuli extend eastward to the Pontic steppe, which is the center of their distribution, while the Baden culture extends farther to the west than the tumuli. In general, the tumuli are found in areas unsuitable for farming but suitable for stock raising. Sherratt suggests that steppe population moved into vacant or abandoned land in the Hungarian and Romanian plain. This is not only movement by a population that should not have spoken Sherratt and Sherratt's proposed koinė; it is a movement that began well before they propose that the koinė was formed.

Chernykh (1980) traces to the Kuro-Araxes culture in the southern Caucasus the source of the new metallurgy using arsenical bronze and bivalve molds for characteristic shaft-hole axes. Beginning in the second half of the fourth millennium, this new metallurgy spread across the Russian steppe into the Balkans and up the Danube valley. He says that these characteristic axes do not show up in any quantity in Anatolia until the late third millennium. This is confirmed and better illustrated in Chernykh (1992). The map in Chernykh (1992: 147) shows that arsenical bronze was firmly established in the southern Caucasus, the Pontic steppe, and the Danube valley in the early Bronze Age at a time when it is only rarely found in northwestern Anatolia. The spread of arsenical bronze into this area is mainly in the middle Bronze Age, which for Chernykh begins about 2700 B.C. (cf. the map on 49). The maps on page 154 show a similar pattern in the distribution of shaft-hole axes. Yakar (1985: 383–84) also argues that the metallurgy of northwestern Anatolia in the early Bronze Age is to be traced to the Ezero-Cernavoda-Cotofeni cultures of southeastern Europe.

I also noted earlier that Sherratt (1983b) says that wool sheep are found in the Pontic steppe, the Balkans, and eastern Europe in the fourth millennium, that the steppe route is the most likely vehicle for the introduction of wool sheep, and that the Corded Ware culture was the vehicle by which wool sheep spread throughout northern Europe. Apparently, Sherratt still believed this in 1994 (1994: 187): "A powerful new culture developed, characterized by pit graves covered by wooden beams and marked by a round mound or kurgan. . . . Small groups of these people penetrated along the Danube into northern Bulgaria and up into eastern Hungary. . . . It was through such eastward connections that ultimately Near Eastern features found their way into Europe other than via the Aegean: wool-bearing breeds of sheep, and a metal casting technology using the two-piece mold"; (1994: 171): "In Northern Europe, fundamental changes which incorporated the full potential of plough farming and pastoralism only took place after 3000 B.C. with the spread of the Corded Ware complex."

It is also true that an economy based on pastoralism appeared first in the Pontic steppe and then spread into central and northern Europe. Sherratt (1982a) documents the arrival of pastoralists in the Hungarian plain in the second half of the fourth millennium.

We thus have evidence that the population movement from the steppe and the spread of wool sheep, new metallurgy, and an economic system based on pastoralism began in the fourth millennium, well before the formation of the Sherratts' version of PIH. Moreover , the source of this movement, as far as central and northern Europe are concerned, is the Russian steppe. This clearly indicates that the Sherratts' version of the spread of PIH could not have been aided by control of access to wool and arsenical bronze, cannot be supported by any known population movement, and followed rather than accompanied the spread of the new, pastorally based economy. All this makes it appear that the Sherratts' cultural arguments for the spread of PIH (really PIE) would work much better if they had located the beginning of the spread in the fourth millennium and if they had identified the geographical source as the Russian steppe, rather than the western end of the Black Sea.

However, to argue that the Yamnaya culture was actually the source of the spread of Indo-European in Europe, we need a better scenario for language spread than that offered by the Sherratts.

I will now turn to a more detailed discussion of the spread of Indo-European into Europe. *Indo-European* is the proper term here since we are discussing a time after the separation of the Proto-Anatolians from Proto-Indo-Hittite. I will focus on the spread into north-central Europe because it was in northern Europe that the Corded Ware culture was formed. No one has yet figured out a coherent linguistic history of Europe without assuming that both the Corded Ware and the Yamnaya cultures were predominantly Indo-European speaking, and yet there is no general agreement about the relationship between those cultures.

I noted in the introduction that the spread of Indo-European is an important fact about the prehistory of Europe and that prehistorians ignore it at their peril. One can always argue that similarities in culture are due to parallel developments or exchange of information among ruling elites. One cannot apply that argument to language. If two culturally distinct areas, distant from each other, speak the same language or languages that are genetically related, then the language of at least one of them did not develop independently in place. This fact can and should color our interpretation of other developments.

The relationship between the Corded Ware and the Yamnaya cultures is controversial. There are many similarities between the two cultures and also significant differences, or they would not be different cultures. All the items of similarity, with the possible exception of wheeled vehicles, appear first in the Yamnaya culture, later in north-central Europe. These similarities include an economy based on livestock breeding without long-term settlements, the use of tumulus burial (less consistent in the Corded Ware), evidence of social hierarchy, cord decoration on pottery, the presence of domesticated horses, wool sheep, and wheeled vehicles. Chernykh (1992: 138–39) suggests that the Corded Ware culture used the casting techniques and bivalve molds associated with the metallurgy that spread from the Caucasus through the steppe. However, it initially used unalloyed copper rather than arsenical bronze.

We thus see a variety of examples of cultural flow from the southeast to the northwest. When we look for evidence of a flow in the opposite direction, we find little or nothing. No case can be made for any cultural spread from north-central Europe into the steppe at any time relevant to the spread of Indo-European.

One might still argue that the Corded Ware culture developed with little contribution from the culture of the Pontic steppe, and respected scholars have (e.g., Häusler 1981, 1983). The economy of north-central Europe was ready for the addition of a mobile pastoral component and could have developed one on its own. A more stratified society may also be the result of internal evolution. The idea of tumulus burial might spread from elite to elite without much movement. Horses and wool sheep might have been acquired by trade, and metallurgical technology could have spread with the movement of very few people.

In that case, however, we must ask why the two areas ended up speaking Indo-European. Again, no case can be made for a cultural spread from north-central Europe into the steppe at any time relevant to the spread of Indo-European. The Corded Ware culture was an economically robust, aggressive, expanding complex, which spread in almost every direction except back into the steppe. All indications are that it had good trade (or exchange) networks with rapid communication and spread of innovations. There is no obvious case where it came in contact with any group in northern or western Europe that was economically, culturally, or militarily superior to it. It seems highly unlikely that the Corded Ware culture, once formed, would have adopted anyone else's language. To account for both the linguistic and the archaeological facts, we must assume that Indo-European speakers played a role in the formation of the Corded Ware culture. This does not necessarily mean that the Indo-European speakers were new emigrants from the steppe, nor does it mean that the Corded Ware culture must have been formed on or near the steppe and then spread throughout northern Europe. It means that Indo-European speakers were on hand at the time and place where the Corded Ware culture was formed.

The Corded Ware culture is apparently the last and best candidate for the kind of influence from the steppe that could have led to the spread of Indo-European into north-central Europe. It was also temporally very close to the first. Since the common vocabulary of PIE includes words relating to wheeled vehicles, the *terminus post quem* for the division of PIE is the appearance of the wheel. There is no good evidence of wheeled vehicles until the second half of the fourth millennium. Even if the wheel had developed earlier, the appearance of wool sheep provides another piece of evidence for the *terminus post quem*. Wool sheep do not appear in northern Europe until the second half of the fourth millennium. The Corded Ware culture dates from about 3200 B.C. In order to have Indo-European speakers on hand for the formation of the Corded Ware culture, we must look for evidence of influences from the steppe in the period between 3500 and 3200 B.C. We should look for evidence in this period for cultural contact between the steppe population and that of central and northern Europe and try to construct a scenario whereby the language of the steppe could become the dominant language. This must apparently happen without a massive, destructive invasion and large-scale population replacement.

The period 3500–3200 B.C. was the period of Gimbutas's second wave of kurgan expansion, which is the first expansion that might be tied to the cultural vocabulary reconstructed for PIE. Good evidence exists for movement of steppe pastoralists into the upper Balkans, up the Danube valley, and into northern Europe in the second half of the fourth millennium. We have seen Sherratt's evidence for people with the material culture and burial habits of the steppe who appear in areas good for pastoralism in the Hungarian plain at that time. These pastoralists apparently coexisted with the local farmers of the Baden culture. Gimbutas notes similar but less well documented phenomena along the Black Sea coast and in the north European plain, also in the second half of the fourth millennium. In the Usatova-Gorodsk-Foltesçti complex along the coast of the Black sea, farmers of the Tripol'e culture occupied wide river terraces, while kurgan people were located on surrounding hills and dunes and on spurs in the river (Gimbutas 1979, 1980, 1997: 251, 274–76).

In northern Europe, beginning about 3400 B.C., we find stock raisers of the Globular Amphora culture, which Gimbutas also attributes to her second wave of kurgan expansion. Again, these people brought the material culture and burial habits of the steppe and coexisted with indigenous farmers. While the farmers were concentrated in areas in river valleys with fertile soils, the herders inhabited areas difficult for farming that were near sources of goods important for trade, such as flint and amber (Gimbutas 1979, 1980, 1997: 254–56, 283–91).

The relationship between the Globular Amphora culture and the Yamnaya culture is considered controversial by some (Mallory 1989: 250–51; Mallory and Adams 1997: 226–27). Gimbutas, as noted above, is certain that the Globular Amphora culture is made up principally of intrusive steppe pastoralists. The similarities that she cites include a pastoral economy with limited transhumance, burial mounds with mortuary houses built of stone, cromlechs, ritual burial of horses, dogs, and cattle, and human sacrifice in burial of prominent males, including suttee.

Their distinctive item of pottery—the globular amphora with corded decoration, is very similar to amphorae found at Mikhailovka I on the Black Sea. Moreover, the Globular Amphora culture covers areas with two previous cultures—the Tripol'e and the TRB cultures, and the TRB extends farther to the west than does the Globular Amphora culture.

The genetic, anthropological features of the males (not the females, a point that becomes important later) in burials associated with steppe expansions, including the Globular Amphora culture, are initially similar to the features of males of the steppe population (Necrasov 1980). These similarities were apparently also evident among the early kurgan burials in Hungary (Ecsedy 1979: 91–93). The similarity decreases with distance from the steppe (Schidetzky 1980, Menk 1980).

Menk (1980) performed a multidimensional computer analysis of cranial measurements of many different populations of eastern and central Europe in the Eneolithic and early Bronze ages. He used the computer analysis to compute morphological distances between populations. While his measurements are obviously limited to skull shapes, his figures show significant differences in the relevant areas of Europe before and after Gimbutas's waves of kurgan expansion. His chart (1980: 382) of computations of morphological distance from the Drevneyamnaya (oldest Yamnaya) population shows the most interesting results.

The chart has arbitrary units, with the maximum difference between populations being fourteen units. In the east, the Yamnaya population is closest (only one unit of distance) to the pit-comb culture, which covered a large area of European Russian from the Baltic to the Urals. On the other side of the Urals, the Siberian Neolithic population is more distant (six units). The Dnieper-Donec/Sredny Stok population is quite distinct from everything else, being a distance of eleven units from the Yamnaya population and fourteen units from the Tripol'e farmers immediately west of them (cf. chart in Menk 1980: 380). Menk characterizes the Dnieper-Donec population as palaeo-europid. Compared to the Yamnaya population, the farming populations to the west and northwest are quite distinct: Tripol'e ten units, Gumelniţa thirteen units, TRB nine units.

After the second kurgan expansion, the Dnieper-Donec population essentially disappeared, according to Menk. In the areas where the Mediterranean-type farmers had lived, we find populations that were much closer to the Yamnaya population. Along the Black Sea the Usatovo and the Gorodsk-Horodiştea-Folteşti populations have a morphological distance of only about three units. Farther west, the Cernavoda culture has a distance of six units from the Yamnaya, but, interestingly, its deviation is more toward Dnieper-Donec characteristics than toward those of the Mediterranean-type farmers who preceded this culture (cf. chart in Menk 1980: 373). In the northwest the Globular Amphora culture in Romania and Poland has a distance of only three units from the Yamnaya. By the time the Globular Amphora culture reaches the territory of modern Germany, however, the Yamnaya influence has disappeared. If more data were available from Poland, we would expect it to show a gradation in characteristics. The oldest area of the Corded Ware culture was essentially the same as the Globular Amphora culture in Poland and Germany. In Poland two measurements for Corded Ware populations are three units (Zlota) and between five and six, while in Germany there is no difference from the original farming population (nine units).

We thus have people appearing in previously uninhabited areas with the material culture, burial habits, and physical characteristics of the people of the steppe. It is difficult not to believe that this represents population movement from the steppe into north-central Europe. Since we know the language spread, and these changes take place at the perfect time to account for that spread, we should assume that the language of the steppe appeared in north-central Europe at this time.

No good reason arises to classify the appearance of these herders as an invasion. We could call it demic diffusion, but demic diffusion of pastoralists, not farmers. A farmer's son on the frontier of the advancement of farming might have to move only a short distance along a river to find new arable land. A pastoralist would likely have to move dozens of miles to find an unused herding territory. This means that pastoral societies, diffusing into unoccupied land, move very quickly.

We thus have very good arguments that pastoralists of the Yamnaya culture moved into central and northern Europe, beginning in the second half of the fourth millennium. Certainly, they brought their language with them. There is no reason to assume that they exterminated the population of those areas and replaced them, but we need to understand why the language of the pastoralists came to dominate. We would also like to explain why the society they formed was so successful and spread so far and so fast.

We know a good bit more about the vocabulary of PIE than that of PIH, and we can get a better picture of their culture. There is reasonable evidence that PIE living units were patrilineal, patrilocal, and exogamous (Delbrück 1889). This evidence is based on ancient texts and on the vocabulary of kinship and marriage. PIE has a set of reconstructed kinship terms denoting the members of a patrilineal group but few if any solid reconstructed terms for a mother's relatives. The word for 'livestock', **peḱu*, may have been used for *property* and *wealth* (Mallory-Adams 1997: 23). This became the word for *money* (or the derivational base of that word) in Germanic and Latin. Thus Old English *feoh* (livestock, property, money), Latin *pekunia* (money). This makes sense in an economy based on pastoralism. After the secondary products revolution—particularly after the introduction of wool sheep—livestock did represent wealth. Domesticated animals could be used to provide labor, food, and clothing without necessarily being slaughtered. A single man tending a herd could produce much more essential produce than could a fourth-millennium farmer. That meant that the more livestock a person or a society owned, the greater its wealth. Even in Indo-Iranian, which does not show a specific meaning of **peḱu* as *property*, the word shows up in expressions of the wealth of a society: thus Avestan *pasu vīra* (livestock and people) is used in a formula for wealth, while Sanskrit *virapśa-* means *abundant of people and livestock.*

Some linguistic evidence suggests that the Indo-Europeans did not always respect other people's property. In Avestan, Greek, and Latin, the verbal stem **aǵe-* (drive), when it takes a domestic animal as object, can mean to drive off that animal as loot. That the same construction was possible in Celtic is shown by the Old Irish expression *táin bo* (cattle raid < **to-ag-nai gwou-*, literally a drive of cattle). Bruce Lincoln (1981) reconstructed a "Myth of the First Cattle Raid" for Indo-Iranian and found traces of a similar myth in European IE languages. Interestingly, a Vedic hymn relating to this myth uses the verb **aǵe-*, although in this case it can be interpreted literally:

> *jó gā́ udā́jad apadhā́ Valásya . . . , sá, janāsa, Índras*
> (he who drove out the cows by the unclosing of Vala . . . , he, o men is Indra) Macdonell, 1992: 46

In this passage the verb *udā́jad* is from **ud-é-aǵe-t* (out-past-drive-3rd sing.).

Lincoln points out similarities between the myth and practice of the Indo-Iranians and those of tribes in Africa with cattle-based economies. This does and should raise questions about the naive assumption that similarities in the mythology of Germanic and Indo-Iranian justify a reconstruction of that myth in a common ancestral society. Nevertheless, the argument remains as a typological argument. If such mythology and practice arise naturally in relatively primitive stock raising economies, then we should not be surprised to find evidence for such practices and mythology in early IE society.

Bogucki (1988) paints a fairly grim picture of the economy of the farmers in northern Europe before the arrival of the pastoral society. There was no shortage of land; the problem was labor, since the production of food through farming was labor intensive. Farmers had small herds of cattle as a hedge against crop failure and for food supplies to get them from one crop to another. The herds were too small to be stable, and any loss of livestock could have been catastrophic. Wild animals did not constitute a significant source of food, probably because game animals had simply moved away from the areas undergoing concentrated farming. Evidence exists of the use of wheeled vehicles shortly before the arrival of the steppe pastoralists, but there was little open space between fields in the loess belts where farming was concentrated. Bogucki expresses doubt that the wagons were used for more than transporting supplies and grain to and from the fields in the immediate area.

The arrival of steppe pastoralists in his area would have provided both a threat and an opportunity for a farmer. On the one hand, a raid on his herd of livestock could lead to starvation before the end of the next growing season, even if none of his family were hurt in the raid. In any conflict, the pastoralists had several advantages. They were more mobile, having horses, so it was easy for them to select targets where they had immediate superiority in numbers and to get away quickly. Moreover, they did not have a fixed base to defend. They could therefore intimidate small farming communities even without attacking them. A modern-day parallel is found in Asia. Zvelebil (1995) notes that in Asia, when semi-nomadic pastoralists come into contact with farmers, they tend to oppress the farmers.

On the other hand, the pastoralists offered the farmer an easy source of new livestock that he could get in exchange for farm produce. The pastoralists also brought with them a new and valuable product—wool. The new technology in metals production would also have been valuable. The herders themselves did not have any real motive to exterminate all the farmers in the area, since they needed grain for their diet. At the frontier of the expansion, the choices offered the local farmers would likely have been rather sharply differentiated: the risk of ruin by suffering raids on their livestock or alliance with the herders, perhaps reaping some of the benefits of raids on farmers farther along the path of pastoralist expansion. This could have led to the rapid expansion of a combined herding-farming society.

Because economic ties are established through marriage in most primitive society, considerable intermarriage likely occurred between the pastoralists and the farming population. I noted above that the kinship structure of PIE suggests a society that practiced exogamy. If the leading edge of the diffusion of pastoralists was made up of single clans, or of single males striking out on their own, the intermarriage at the leading edge of the expansion could have been almost complete. I also noted above that while the males in the kurgan burials typically have the physical traits of steppe populations, the females do not. This could explain why the genetic traits of the steppe population diluted rapidly as the pastoral society moves farther into northern Europe.

We could argue that in the second half of the fourth millennium—at least temporarily—the areas of Europe adjacent to the steppe formed good candidates for what Johanna Nichols (1998) has called linguistic spread zones, that is, large open areas with low population density. Controversy continues, however, over whether the depopulation of the upper Balkans was due to an invasion from the steppe (Gimbutas's first wave) or to internal events such as change in climate or salinization of the soil. For our purposes it does not matter. The depopulation had occurred, so there was plenty of land with very little population. Thanks to Sherratt (1981, 1982a), we know that prior to the appearance of the steppe herders in the Hungarian plain a massive reduction in the number of farming sites occurred—almost certainly due to salinization of the soil. In northern Europe there is no similar research to indicate significant abandonment of sites due to over farming, although there must have been some. Huge areas in the north European plain were empty, however, except for sparse populations of hunter-gatherers. Pastoralists could have moved in without any initial obstacles. Both the pastoralists and their language would have found these areas convenient spread zones.

Consequently, we have a picture of a rapidly expanding pastoral society, which could combine and intermarry with the local population and introduce an economic system considerably wealthier than the preceding culture. As Sherratt says, this society was geared to take full advantage of the secondary products revolution.

In terms of language, we would expect that intermarriage and the fact that farmers had to deal with herders for essential items like wool and metal products would lead to considerable bilingualism. Since the pastoralists had spread far and fast, while the farmers had been in place for a long time, we would expect that the pastoralists' language would be more consistent over a large area, while the farmers' languages would be broken up into many dialects and distinct languages. Since wool sheep, new metallurgy, and domesticated horses were introduced from the steppe, we have reason to suspect that the new arrivals could have maintained control over access to these goods. Thus, the language of the steppe people would have been a much more useful language to know. The exchange networks that brought wool sheep and horses to the area were initially controlled by speakers of the language of the steppe; so we should expect that, when exchange networks evolved into trade routes, the language of trade would be the language of the steppe pastoralists.

What this means is that instead of spreading because it was a trade language, PIE became a trade language because it had spread. The facts that PIE was spoken over broad areas with less variation and that it was associated with a society both militarily and economically superior to the competing systems made PIE a logical candidate for a regional language, when the need for such a regional language developed.

If this scenario for the spread of PIE into northern and central Europe is correct, then we are in a position to offer some justification for the claim that the division between PIE and PA occurred before the arrival of wheeled vehicles. The physical and cultural separation of the PIE speakers in the Yamnaya and the Corded Ware cultures provides the prerequisites for linguistic differentiation within the PIE branch of PIH. Because cultural differences had been established by 3200 B.C., the time of the beginning of the Corded Ware culture, we can assume that linguistic differences were also developing. Because any differentiation in the PIE branch must follow the establishment of PIE by the division between PIE and PA (including time for any common PIE changes), we have a second line of reasoning that posits a *terminus ante quem* for the division between PIE and PA in the second half of the fourth millennium B.C. And because evidence of wheels in the steppe also occurs in the second half of the fourth millennium, we can assume that dispersal of PIE speakers started shortly after they acquired wheeled vehicles.

Finally, we can return to the issue of the time and place of the division between PIE and PA. The Yamnaya culture is dated by Mallory (Mallory and Adams 1997: 651) from 3600–2200 B.C., and there is broad agreement that it developed from the Khvalynsk and Sredny Stok cultures in the same area. Because these cultures existed without strong external cultural influences for several centuries before the occurrence of evidence for wheels, there should be a strong presumption that the Khvalynsk and Sredny Stok cultures are strong candidates for the home of PIH.

Do we have evidence for expansion of the steppe culture toward western Anatolia before the introduction of the wheel? Yes. The sequence of depopulation followed by the appearance of people with the economy and burial habits of the steppe began in the fifth millennium in the Balkans, and the subsequent close cultural interaction across the Bosporus, which Sherratt and Sherratt use to justify the formation of PIE, could be evidence of the kind of contact that we would need to explain the spread of PA into northwestern Anatolia. Iarovoi (1985: 106–12) documents steppe-type burials in the area of the Black Sea east of the Danube from the time of about 3500 B.C. (uncalibrated). Wheels and wheeled vehicles occur in groups that he attributes to the second half to the third millennium. His dates are not based on carbon 14 measurements but on comparison to burials with uncalibrated dates elsewhere. In calibrated dates, this would indicate the appearance of steppe burials about 4400 B.C., with the appearance of wheeled vehicles about 3200 B.C. As late as 3500 B.C. (calibrated) we find the Usatovo culture along the Black Sea east of the Danube, which is described as a hybrid culture containing elements from the steppe and from the Tripol'e culture (Mallory and Adams 1997: 614). Bökönyi (1987) describes this culture as having had both the tall sheep, which he associates with woolly coats, and the shorter Neolithic-type sheep. They definitely had horses as well (Potakova 1979: 145). There is no direct evidence of ox traction, but since Gusev (1998) notes the existence of model sledges on runners in the Tripol'e culture as early as 4000 B.C., we can hope that ox traction existed. If Usatova was indeed a hybrid culture made up of steppe pastoralists and Tripol'e farmers, the language of the steppe group could easily have already become separate and differentiated from that of the rest of the steppe. The local farmers would have had the same motivation—the same choices for survival—as the farmers of central and northern Europe to adopt the pastoralists' language. The same could be said of other hybrid cultures farther west along the Black Sea, such as Cernovoda and Ezero. One of these may have produced Proto-Anatolian.

Close cultural ties across the Bosporus were established in the fourth millennium. I mentioned earlier that Yakar attributes the metallurgy in northwestern Anatolia to influence from the Balkans, and horse bones are found at Demirçi Hüyük in the fourth millennium. Demirçi Hüyük is on the most logical route leading from the Bosporus to the central Anatolian plateau, where Hittite was historically located. A third millennium sequence of destruction and depopulation, followed by a switch to a more pastoral economy, covers the area where Luvian was historically distributed. (Mallory 1989: 28–29).

The Bosporus is probably not the locus of the original separation between PIE and PA, although it may have been the locus of the final separation. The linguistic separation was probably started with the establishment of hybrid cultures in the Balkans. Anthony (1991b) argues that the original boundary between the Tripol'e farmers and the horse-oriented steppe culture was the Dnieper. On that basis we could speculate that the place of the division between PIE and PA was the lower Dnieper, and the time was between the arrival of ox traction and the arrival of the wheel, probably in the first half of the fourth millennium.

Notes

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1. An alternative explanation for the lack of a prothetic vowel in Greek is offered by Martin Peters, *Untersuchungen zur Vertretung der indogermanischen Laryngale im Griechischen, Wien: Verlag der österreichischen Akademie der Wissenschaften,* 1980, 23n. 18. Following a suggestion by Rix, he proposes that word-initial laryngeal disappeared before w plus a following syllabic liquid. This works not only for the word for *wool,* but for a variety of other forms.

2. To get these percents I combined the data from tables on pages 75 and 85, weighting the percentages by the number of individuals given in appendices 5 and 7, pp. 157–63.

3. Parallel examples are Lith. *bradas* (ford), *bristi,* [past] *brìdo* (wade), *skradnus* (fast flying), *skristi,* [past] *skrìdo* (fly) (Skardžius 1943:473–74).

4. Craig Melchert, in comments on this paper, pointed out the existence of a verb meaning *drive* in Hieroglyphic Luvian of the shape PES2 *wa/i-za-*, which could be from **wegh-j-e/o-*. While not a direct cognate of the word **wegh-e/o-*, it is certainly a possible instance of the root.

5. I discussed this in detail in an article, "Indo-Hittite and the Caucasus," which will appear in the proceedings of the Chicago Conference on Caucasia, May 1999.

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CRITICAL RESPONSE TO THE LAST FOUR PAPERS

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I am grateful to Robert Drews for inviting me to play a role in this Colloquium. I particularly welcome this opportunity because I feel that the issues addressed here have not always received, from what I have termed the "Indo-European establishment," the full attention and respect that they deserve. I trust that all will understand that I cannot do full justice to these four data-rich papers in the time available to me. I can only give a reasoned critical evaluation of the analyses and arguments presented—within the limitations of my own expertise—citing enough specific points to justify my assessment.

I begin with the paper of Margalit Finkelberg on the language of Linear A, which stands somewhat apart from the others in both subject matter and methodology. A marked strength of her presentation is the logical and linear progression of the argumentation. She first presents evidence (such as apparent matching word pairs between Linear A and B) to support the generally accepted phonetic values of the syllabic signs of Linear A. While a full decipherment might bring some refinements, it seems that we must take the established interpretation of the syllabic signs as essentially correct. This point is of fundamental importance, since it effectively precludes any solution to the problem of the language of Linear A along the lines recently successfully advanced for Carian: i.e., a radical revision of the sound values assigned to the individual signs. We must essentially deal with the language we have.

Professor Finkelberg next tries to establish what she terms a phonological and morphological "profile" of the language of Linear A. She then compares this with the profiles of various languages of the Eastern Mediterranean that have been or might be proposed as candidates for the language of Linear A, very effectively reducing the number of reasonable possibilities. While one must always allow for the possibility of under-representation of linguistic distinctions in a writing system (and even for the existence of a typologically unusual member of a given language family), her arguments against Linear A as representing a form of Greek, Semitic, or Hurrian are convincing.

Having shown that the "profile" of "Minoan" is compatible with that of an Indo-European Anatolian language, Professor Finkelberg then (and only then) turns to specific comparisons. In this step one can only concur with her insistence on what she calls "methodological coherence." The comparison must be systematic; it is improper to match up individual features of the target language, Minoan, with *randomly* selected features of various Indo-European Anatolian languages. Unfortunately, her analysis does not always meet her own standard in this regard. For example, the putative order of the enclitic animate accusative singular pronoun and the reflexive particle in Minoan (reflexive *-ti-* followed by accusative *-nu*) actually matches that of Luvian (*a/wa-ti-an*), *not* that of Lycian (*-ẽ-ti*) or Hittite (correct X-*an-za*!). While Lycian has several "particles" containing a sequence *-we-*, neither the word order nor the distribution of any of them supports an equation with the quotative/introductory particle *wa* of Hieroglyphic Luvian. Finally, Finkelberg makes much of the presence of an *e*-vowel in choosing to associate Minoan with Lycian. However, in the only two specific equations made (the dative singular pronoun *-ija* and the preterite third singular ending *-t/da*), Minoan would show an *a* like Luvian, versus Lycian *e*. It is by no means impossible to account for this discrepancy, but if Minoan /e/ does not actually match Lycian /e/ in its distribution, then the significance of the typological agreement in vowel systems seems badly compromised. If second-millennium Minoan is to be in any sense a direct forerunner of first-millennium Lycian, then one would like to see some account of why some cases of Lycian *e* (which Finkelberg accepts as coming from prehistoric **e* and **o*) show up as *a* in Minoan.

I stress that such problems are not in themselves fatal to Professor Finkelberg's basic thesis. I personally find it a priori unlikely that a language found on Crete in the eighteenth to fifteenth centuries B.C. would turn out to be an exact match for one of the attested Indo-European languages of Anatolia. I would expect rather to find yet another dialect with its own peculiar combination of features. One conceivable possibility, for instance, would be a "western" dialect with the grammatical features of Luvian and Lycian, but where **o* has merged with **a*, leaving a four-vowel system comparable to that of Hittite (the putative examples cited above, *-ija* and *-t/da*, would reflect **o*, not **e*).

A much more serious problem for me is the proposed content of the opening formulae on the Minoan libation vessels. What is the first (or only) sentence on such an object likely to say? The one thing I am sure of is that it will *not* consist of the conditional clause of a prohibitive formula, as Professor Finkelberg suggests. I know of no text in any Indo-European Anatolian language that begins in such a fashion. It is true that we have only a few comparable votive inscriptions on objects; however, all of those that we do have begin with (or consist entirely of) a sentence referring to the object itself and either its manufacture or its dedication. An admittedly cursory examination suggests a similar pattern for Greek votive inscriptions.

A further difficulty concerns the "chains" of introductory particles and pronouns. Such chains are indeed a famous marked characteristic of the Indo-European Anatolian languages, but the function of all of these elements is in the largest sense anaphoric: they refer back to previous elements in the discourse. Therefore, the one place where they typically do not appear is precisely in the first sentence of a text. There are exceptions to this generalization, but they are of a limited and very specific kind.

In sum, therefore, I find Professor Finkelberg's approach methodologically sound and her analysis promising, but I remain unpersuaded by the specific comparisons made in the absence of a more plausible account of the content and overall structure of the opening formulae of the Minoan texts.

* * *

I will next treat the papers of Professors Ivanov and Lehrman together, since they touch upon several common themes. I will focus first on the arguments of Professor Ivanov for "Southern" and "Northern" Anatolian as two separate Indo-Hittite dialects, represented chiefly by Luvian and Hittite respectively. In terms of methodology, I am not as adamantly opposed as Professor Lehrman to alternatives to the Stammbaum model. In principle I am quite prepared to apply notions of a *Sprachbund*, areal features, or convergence to the languages of ancient Anatolia. For example, I think that the devoicing of initial voiced stops in the Indo-European languages of Anatolia is an areal feature (probably shared with Hurrian) that spread gradually across these languages, not a single change that had taken place already in Proto-Anatolian. I also believe it is always salutary to reexamine one's basic assumptions, and this includes the very notion of a single intermediate prehistoric Proto-Anatolian as the source of all the attested Indo-European languages of Anatolia (except for Phrygian, of course). Despite a number of published statements on my part to the contrary, I would not *absolutely* preclude that Lydian, which is in many ways very different from the rest of the Anatolian group, reflects a separate tradition whose shared resemblances to the others are due to late convergence.

I cannot, however, accept Professor Ivanov's formulation of two separate branches of Indo-European Anatolian divided into Southern and Northern (or Western and Eastern). I cannot discuss the lexical differences cited between Luvian and Hittite in detail. I will say only that comparable divergences can be found in closely related languages elsewhere, such as Irish and Welsh. I remain unimpressed by his statistical analyses. Even if one accepts the glottochronological premises on which they are based (which I do not), our ignorance of a large percentage of Luvian core vocabulary (a point he acknowledges) renders such statistics meaningless.

As to phonology, I must once again vehemently protest the misuse of the term *satem* to describe the development of stops in Luvian and Lycian. The significant feature of the development referred to as "satem" is not the trivial phonetic change of the PIE palatal stops into fricatives and affricates, but rather the (generally unconditioned) merger of the PIE labiovelar and velar stops. No such merger takes place in Luvian and Lycian! Furthermore, even the phonetic development in these languages is not comparable to that in the true satem languages. In Luvian and Lycian only the voiceless *ḱ becomes an affricate *ts* *unconditionally.* The treatment of the voiced series is a complex mix of conditioned palatalization and loss. We are thus left with Luvian and Lycian (and probably Carian) sharing in the trivial affrication of the voiceless palatal stop *ḱ, one of several common innovations shared by these languages but having no more significance than any other.

I must also reject in the strongest terms the claim of a "quasi-ergative" value for the enclitic marker *-sa/-za* attached to the neuter nominative-accusative singular in Luvian. This marker occurs more than seventy times in the Cuneiform Luvian corpus. Not a *single* example can be shown to be the subject of a transitive verb. Most examples are in fact functioning as direct objects. A few occurrences as the subject of intransitive verbs are of no significance because there is no restriction in the Anatolian languages, any more than elsewhere in Indo-European, on grammatically neuter nouns as subjects of intransitive verbs. There is absolutely *no* evidence for any "active" or "personifying" force in the *-sa/-za* marker. On the contrary, both Luvian and Lycian do show the true ergative marker **-anti-* matching that in Hittite, one of the most significant common innovations establishing a unitary Proto-Anatolian.

The most serious problem with Professor Ivanov's proposal is that there is no consistent division between "Southern" and "Northern" Anatolian, as we would expect by his formulation. For example, in terms of the preterite first singular ending *-ha* and preterite third plural *-nta* that he cites, Palaic, a Northern language, groups with Luvian and Lycian. On the other hand, Lycian, a Southern language, has a genitive plural *-ẽ* and dative-locative plural *-e* matching Hittite *-an* and *-aš*, not the renewed endings of Luvian. Hieroglyphic Luvian has a nominal genitive singular in *-as* agreeing with Hittite and Palaic, along with the relational adjective in *-assa/i-* characteristic of the western Anatolian languages. In sum, the overlapping shared innovations among the various Anatolian languages point not to two completely distinct branches that have later undergone convergence, but to a set of closely related dialects differentiated by a typical pattern of competing isoglosses.

Ivanov also ignores or under-appreciates the significant common innovations shared by all the Anatolian languages, as stressed by Lehrman. I will cite only two here. First, there is the "*hi*-conjugation," which, as per Lehrman, *in its attested form* is an innovation of Proto-Anatolian, not a frozen archaism of Proto-Indo-Hittite. Second, there is the medio-passive in *-r*, which again in its specific features shows Proto-Anatolian innovations. It is worth noting in regard to both of these categories that they are recessive in the attested Anatolian languages. This fact argues decisively against their shared appearance in these languages being due to convergence. For these and other reasons I must therefore respectfully reject Professor Ivanov's analysis.

I should preface my remarks on Professor Lehrman's paper by making clear to non-specialists that he and I have radically different views regarding the specific linguistic features of Proto-Indo-Hittite and Proto-Anatolian. We also have some serious differences in terms of methodology. (Like others at this colloquium I cannot share his views on the status of linguistic archaicity vis-à-vis the absolute age of textual witnesses). However, he and I are in substantial agreement on the crucial points affecting the theme of this colloquium.

Leaving theory aside and focusing on achieved results, I can only agree with him that at the present time the only thing that may be regarded as demonstrated is a first division of Proto-Indo-Hittite into Proto-Anatolian and Proto-Indo-European. I use here the terminology adopted at this colloquium. As the late Warren Cowgill observed, it is of decidedly secondary importance whether one labels the oldest common stage as above or calls it Proto-Indo-European and creates a new term for the next stage that does not include Anatolian. The crucial point is that limited but compelling evidence now shows that the rest of the Indo-European languages underwent a set of shared common innovations in which Anatolian did not share.

A consensus on this point has been developing in the field for at least the past decade, although Professor Lehrman seems to have discovered the development only recently. I do not, of course, mean to say that there are no dissenters to the majority view. Nor is there any broad agreement on just how radical the innovations of non-Anatolian Indo-European are, or on what the correct set of innovations is. In this regard Professor Lehrman and I are at opposite ends of a broad spectrum of opinion. But the notion of Indo-Hittite (or alternatively of "early separation" of Anatolian from Proto-Indo-European) is now growing in dominance. While a number of scholars also have specific views on further chronological "layerings" of the Indo-European dialects, I agree with Professor Lehrman that these all remain at present under debate.

I close my remarks on Professor Lehrman's paper with a few other methodological points. I agree with him that the key criterion for establishing subgrouping remains shared common innovations, indeed, those of a type non-trivial enough to preclude independent parallel developments. While the shared common innovations of the non-Anatolian languages are crucial for establishing Indo-Hittite, I must stress with Lehrman that Proto-Anatolian itself is also positively characterized by a number of significant innovations, like any other subgroup of Indo-European. Neither Hittite nor Proto-Anatolian reflects an unaltered Proto-Indo-Hittite. Unlike Lehrman, however, I find no basis for supposing it more likely that the rest of Indo-European moved away from the Anatolian group than vice versa. I stress that the *linguistic* evidence for Indo-Hittite is by its very nature entirely neutral in this respect. Such a "genetic split" shows only separation/isolation of groups of speakers sufficient to produce different sets of innovations; it cannot tell us anything about the direction or extent of population movements.

I personally find Lerhman's approximate datings (division of Proto-Anatolian by 3000 B.C., division of Proto-Indo-Hittite ca. 4000 B.C.) reasonable, but my evaluation, like his own figures, is based simply on my sense of probability as an experienced historical linguist and nothing more (I reject with him the claims of glottochronology). With an eye to the thought of this colloquium, I doubt that we historical linguists control the rate of language change/differentiation sufficiently for this factor to be a useful argument in debates about homelands or prehistoric population movements.

* * *

My comments on Professor Darden's very interesting presentation will be brief, since I do not have the expertise to evaluate the archaeological/realia aspects of his analysis. I must say, however, even as a nonexpert I was struck by the independent convergence of his and Professor Barber's arguments concerning wool. His concluding scenario for the time and place of the separation of Proto-Anatolian and Proto-Indo-European seems to me highly plausible from the point of view of the Anatolian languages and their first historical appearance. I note in particular the implication that the place of Proto-Anatolian linguistic unity may have been outside Anatolia. Hence not all observed divergence among the Anatolian languages need be attributed to the period of their spread across Anatolia. Speakers of (Indo-European) Anatolian may have entered Anatolia already showing dialectal divergences. This point is obviously relevant in evaluating the attested differences between Hittite and Luvian cited by Professor Ivanov.

I find the linguistic component of Professor Darden's arguments both sound and very promising, though I see some quite non-trivial inherent limitations. Let me say preliminarily that he has eloquently explained why the traditional attempts to use the reconstructed lexicon (notably for flora and fauna) to elucidate the "homeland" problem have failed to reach any satisfying results—and why in principle such attempts are unlikely ever to succeed. (Indeed, in historical terms I find one of the significant features of this colloquium the fact that such arguments have played virtually no role in the proceedings.)

I would like to stress two points that I view as critical in using reconstructed vocabulary as a dating criterion in the manner Professor Darden proposes. First, as he himself points out, one must be able to reconstruct an actual *word* for the relevant proto-language; mere root equations among the various languages are not sufficient. He cites the example of Hittite *ḫurki-* and Tocharian A *wärkänt-*, both *wheel* from the root *_$h_2$werĝ-_. All that this comparison proves is that Proto-Indo-Hittite had a root meaning *turn*. It in no way establishes a word for the notion *wheel* in the proto-language; that the two traditions both created the word from such a root independently (and with different suffixes) once they did meet with the concept would be a trivial occurrence. Likewise, contrary to Professor Darden's assertion, Anatolian does have a reflex of the root *_weĝh-_ (travel/carry by conveyance) in Hieroglyphic Luvian (PES$_2$)*wa/i-za/i-* (drive [a chariot]). However, one point on which even Professors Ivanov, Lehrman, and I can all agree is that the Luvian stem certainly is *not* a reflex of the simple thematic stem *_weĝhe/o-_, reconstructable for (non-Anatolian) Proto-Indo-European. It thus forms no word equation with Sanskrit *vahati*, Latin *uehō* etc., and the attested meaning may easily be an independent development (a renewal of an older meaning, *convey by sledge*, when wheeled traction became available). Thus this example also does not contradict Professor Darden's conclusion that there is no solid evidence for wheeled transport at the stage of Proto-Indo-Hittite.

I would add a second even more stringent criterion for a reconstructed lexeme to be secure as a dating criterion. The morphological formation of the reconstructed word must be recessive in at least two of the crucial traditions (in the present case in Anatolian and at least one other). The reason for this stricture is that if the formation of the word is of a productive type we cannot entirely exclude its being independently created in the different descendant languages.

Of the lexical items discussed by Professor Darden, only *horse*, *yoke*, *bovine*, and most crucially *wool* clearly meet both criteria. The word for *harness pole* is somewhat less secure due to uncertainties about its morphology. Hittite *ḫišša-* matches Sanskrit *īṣā́-* (the few Hittite spellings with *e* have no probative value), but their relationship to Avestan *aēša-* and Greek οἴᾱξ- is anything but clear. The status of the verb *to harness* must be regarded as quite uncertain (a **ye/o*-present would be a trivial innovation in both Indic and Anatolian).

The limitations on Professor Darden's approach lie in the available data. The number of usable vocabulary items from the "secondary products revolution" will be limited in the first place. When we then add the two strict but necessary linguistic requirements cited above, we are inevitably going to restrict the usable data set to a very few items. We are then likely to be left in the uncomfortable situation of basing grand conclusions on extremely limited evidence. My overall reaction then is: this is an excellent start—can we find *any* more examples of the same sort that support the same conclusions?

DISCUSSION SESSION

SUNDAY MORNING

[Craig Melchert's expression of appreciation. (Applause)]

DREWS: Thanks very much, Craig, and thanks to all of you. I must say that I have enjoyed this whole thing, first the planning and now the reality. I'm sorry to say that with Craig's critique we have now reached the end of the planned program, but we now have a fine opportunity—about fifty minutes—for discussion. This is the time to pull things together, if anyone wants to venture something about that, or for challenges, or questions on particular points.

RENFREW: I'd like to take up the "challenge" now—if I may, please—and offer one or two comments on Bill Darden's paper, with the second half of which I am substantially in disagreement. Perhaps I should move a little bit forward; I don't want to have my back to Bill Darden while I'm speaking. It might be a dangerous place to be [laughter].

I would like to say first of all that I fully accept the linguistic points which he made. I thought the short part of his paper, which was primarily addressed to the linguistic distinctions, and the identification of terms in Proto-Indo-Hittite and terms in Proto-Indo-European makes excellent sense, though it's not for me to say so. I'm not a linguist, but I also think it makes excellent sense in archeological terms. What I thought was secondary, if I may say so, was not so much the secondary products revolution as the archeology which we were offered, which was a reading of Andrew Sherratt's very interesting work on the secondary products revolution. But it is important to say that these days most archeologists—including, I think, Sherratt himself—regard the secondary products revolution as a process of intensification, as a number of things came into play in the later Neolithic, I think, rather than as a movement of people. I rather thought we had been presented with something of a rehash of Gimbutas's kurgan model dressed up in secondary products terminology, but that perhaps is not very helpful to just make those comments. I think there were a number of conspicuous errors. For instance, just one point of detail, the development of metallurgy. Copper metallurgy began in the Balkans. It began in the Copper Age, not surprisingly, and as early as 5000 B.C. The whole Copper Age phenomenon absolutely coincides with what Marija Gimbutas called Old Europe. Marija Gimbutas would certainly not have disagreed with that—nor, indeed, would specialists like Chernykh—and so the spread of copper metallurgy was from the Balkans to the steppe lands and then, of course, there was very significant Bronze Age or, perhaps quite often, later Bronze Age metallurgy in the steppe lands, which is of the greatest importance. That can't be denied. Another point of detail, I'm very curious about the camel hair in Romania. Was that third or second millennium B.C.? I would be very interested to have a reference for that, although it is a rather specific point and not an important one.

But what I would rather discuss, if I may have just a couple of minutes . . . I basically accept the whole chronological implication of the wheel/wagon complex. Indeed, in my remarks yesterday I pointed out Lehmann's comments on Specht, and the whole wheel/wagon complex is something that emerges in the fourth millennium B.C. and very probably emerges in the eastern part of that area. And it probably does begin somewhere in the area of Tripolye culture or a little further to the east in the fourth millennium B.C., so I think we're in agreement on that.

It's important to remember that the spread eastwards, on to the steppe lands, was simply the spread of agro-pastoralism. It wasn't the secondary products revolution particularly; it was the spread of agro-pastoralism eastwards on the steppe lands and there I think everybody is very much in agreement. But let's remember it was quite late, the site of Arkaim for instance, east of the Urals, which dates from about 2000 or 1800 B.C., and it's there, and on sites like Sintashta that you find the first wheeled vehicles east of the steppes.

But the point I'd really like to address myself to is the Proto-Indo-Hittite and the terminology there, and I would argue that just about all of that terminology would be perfectly at home in the Anatolia of the seventh and sixth millennia B.C., with one very important point, and here I very much accept Elizabeth Barber's point that wool became important for textiles at a later stage around the fourth millennium, perhaps, with the development of the woolly sheep. But I would have to argue, and I do therefore argue, that the term *wool*, the root *wool*, *lanas*, *lena*, *lana*, whatever it may be, meanwhile may well have had a much earlier meaning in Proto-Indo-Hittite, meaning broadly, the *hair of the sheep* or indeed the *fleece of the sheep*. Now I don't think it's a very big semantic change; I don't think it's very difficult to imagine. Of course, it's speculative, but, of course, we're all familiar with small semantic changes, that when you get the woolly sheep—and I very much accept the chronology of Dr. Barber, which is very much in harmony, I think, with that of Sandor Bökönyi, to whom Dr. Darden was referring—then you may well find that what was previously the term for the *hair of the sheep* becomes the term for the *wool of the sheep*. It's very adjacent semantically.

If we accept that point, then I can accept everything, I think, in Bill Darden's description; everything there would be perfectly at home in the Anatolia of the seventh and sixth millennia B.C. It's true that we don't have very good evidence for ox traction. Plenty of evidence for oxen, but where *do* we have very good evidence of ox traction before the wheel? When the wheel comes in, *then* the oxen are pulling carts, and we are in agreement broadly about the chronology for the pulling of the oxcarts. Before the arrival of the wheel there is very little evidence, and anyway I'm not sure there is any evidence anywhere for ox traction which actually predates the appearance of the wheel. Well, there may well have been ox traction; there may well have been sledges; there may well have been the use of the plow (the story of the plow is not well documented) very widely at a very early date.

But it would have been more impressive, I think, if Dr. Darden had also given us the Proto-Indo-Hittite for, or rather the Proto-European, and spoken to us about *plow* and *milk* and so on. And also if he had given us the Proto-Indo-Hittite for *sheep* and *goat* and *wheat* and *einkorn* and *emmer* and *barley*, because all of these are features that are universal to the areas we are speaking of, whether talking in Proto-Indo-Hittite times or Proto-Indo-European times. Nobody, I think, doubts that the Proto-Indo-Hittites and the Proto-Indo-Europeans were familiar with all the basics of mixed farming; oxen, certainly; sheep, certainly; goats, certainly, and, as I say, wheat, barley, einkorn, emmer, flax. That's well documented, lentils and all these terms. And so if you're going to have a proper analysis, we ought to find these terms introduced. And I'd encourage Dr. Darden to introduce them.

But all I'm saying, really, is if we look at this word—*lanas*, or *lenas*, or whatever, this word for *wool*—if we allow that this word may well have existed in Indo-Hittite referring to the *hair of the sheep*, and the *fleece of the sheep*, then I think there is absolutely nothing in Dr. Darden's description which in anyway is antagonistic to the farming dispersal for Proto-Indo-Hittite. And then we're very much in agreement, I think, about Proto-Indo-European in that Balkan area extending as far east as the steppe lands. I apologize for speaking rather long on the point, but I wanted to take up on the word "challenge" that Robert Drews has offered and put it into application. Thank you.

IVANOV: First of all, about this word for wool. You know, we have some evidence for semantics. You know that in Hittite this *hulana* is also the name of a river which can be rendered by a Sumerian sign, SIG (wool), + a phonetic element, *-na*. So it is connected with water, but in what way? In Slavic, for instance, there are two meanings: the first is *wool*; the second is *wave*: so the wave, as surface of water. And if you look at the sheep, of course, this metaphor, it is quite evident. And it seems to be confirmation of what, Colin, you just suggested. If we have surface of the water, that is *wave* and that is metaphor for the *hair of the sheep*, then, we have the secondary development. So, I think that it can be proven by the existing language; so, it's not simply a hypothesis.

And about the plow, as Colin mentioned it now: I would like to draw your attention to an excellent book written by a linguist who was interested in archaeology, and ethnic sciences, André-Georges Haudricourt (Haudricourt and M. J. B.

Delamarre. *L'homme et la charrue à travers le monde* [Paris: 1955]). This is a history of *plow*, in connection with languages. There are maps there that completely exclude this steppe area. Because as Haudricourt has shown, the history of *plow* shows that it started in a much more southern area, you know, so that I think this is much more important for the whole problem that we are discussing.

Now, two methodological remarks concerning, mainly, the very interesting talks that we heard by Professors Lehrman and Darden. First, I think that gaps may be not in languages but in our knowledge. And this is very important to differentiate. By the way, I also met with this in my attempt to give lexical statistics for the Hittite-Luwian differences. We know only a small part of the lexical evidence. So we should always think about two different possibilities. There is a possibility that some word or some notion or object did not exist, but there is also possibility that we do not know. I had the privilege to know our brilliant scholar in the field of Egypt and general ancient languages and history, Vasilii Vasil'evich Struve. He participated in his youth in the compilation of the Erman-Grapow dictionary. And he told me he always had quarrels with his German colleagues because they supposed some parts of the human body did not exist [laughter] because they were absent in Old Egyptian materials. So we should be quite attentive about this, you know.

And the second, also very important, problem is absolute chronology. Professor Lehrman denied the importance of Tocharian. I will speak against this, because we have the evidence of Lithuanian. Lithuanian is much younger than Tocharian. Lithuanian paradigms are practically identical to Sanskrit paradigms. You may look at it: Old Lithuanian nominal paradigm is not very different from Sanskrit. And there are many forms in Lithuanian that are not different from Proto-Slavic, although we know that Proto-Slavic existed one millennium before modern Lithuanian. So, absolute chronology has nothing to do with archaism. A language, or some part of the language, may be very archaic, although it is known only now. Morgenstierne discovered Nuristani languages that have extremely archaic forms. Some forms of Mitanni-Aryan numerals more resemble Nuristani forms than all the rest of Aryan. So, I think that we should be very cautious about using absolute chronology.

Now one remark of general character—I'm trying to be general, just to conclude our session—that maybe I am representing here a kind of unofficial ideology of the former Soviet Union. In our country, all social sciences, as you know, were very much interested in the material aspect of life. And it seems strange to me that I'm finding something similar in other countries, in other tendencies, maybe due to the legacy of the twentieth century of modern pragmatic values that are completely different from ancient societies. So, I'm very much amazed at the fact that we speak so little about religion, about mythology, about the problems that were so important for all these societies. Let me pick one part of the whole problem of Indo-European migration. If you look at all these maps, the excellent map that Bill has shown us today, what is remarkable is that Iranian takes up so large a part of the whole map.

And I will disagree with the concept of some of my Russian colleagues that tries to find some material ways to understand why Iranian moved so far and so quickly.

Let me give you one concrete lexical example. We have in a Hurrian-Hittite bilingual text published by the late Neu the Hurrian term for fire that is translated by the Hittite word *pahhur*, and this is *tari*, and this is definitely Iranian (not Indo-Iranian, but Iranian borrowing, from *athar*. By the way, *athar* in Sanskrit (in *atharvan*, and *Atharveda*), as Benveniste proved, was also an Iranian borrowing. We have an enormous area of religious influence of Iranian, just over the Iranian fire cult. There are many books about this, written mostly in 1920s and forgotten since then. But you cannot explain the spread of Iranian; the influence of Iranian on several Indo-European dialects that are in a certain way Iranicized. I think Armenian is a very good example; it has so many Iranian terms. Bill Darden was speaking about Tocharian and Iranian, but, you know, the number of Iranian borrowings into Tocharian is amazing. We seem to have some kind of Iranian-Tocharian bilingualism in Chinese Turkistan. And so I think that we should pay much more attention to nonmaterial aspects of life in ancient societies. Otherwise, we will contradict historical reality. This is what I wanted to say in general, but may I add a few words more about very interesting points made by Craig Melchert?

I think that in general I feel privileged by being criticized by the best, I think, specialist in America on Anatolian. I think that the main difference is not so much in facts but in approach. I think that in comparative linguistics we should differentiate between two different parts. The first is the construction of a formal system, and this has been done by Craig in his beautiful book. But then you may interpret this, I think. Take the terms from mathematics. One aspect is formal structure, formal system, for instance the formal system of correspondence, of isoglosses; the other is interpretation. So you may interpret the same system as pointing either to a protolanguage or as pointing to *Sprachbund*. This is a different approach that does not contradict the bulk of the facts.

Now the lexical differences. Now, as I told you, I had the large problem with our knowledge of lexical materials, so I even calculated the percent of Hittite and Luwian and Lycian material not known to us. So many items we do not know. But in the glottochronological lists of one hundred and two hundred items, according to what we have, in the list of one hundred the percent of disagreement between Hittite and Luwian is 19 percent. So almost a fifth part of one hundred basic vocabulary—the most basic vocabulary—is different. In list of two hundred we have 12 percent. You may compare this to published percents of languages that we know much better. So these percents of differences are there.

I will not speak about details, but in general, labiovelars *are* represented in Armenian and Albanian. That was shown by Pedersen. So in general, absence of labiovelars (that is also not absolute in Sanskrit and in Proto-Slavic), this is not an important part of the whole *satem* problem. So in this I disagree. As far as ergative is concerned it is very important that besides transitive verbs we have active verbs.

So verbs like *to go* also need have this ergative construction. And I think all other remarks are very interesting, and I am really obliged for criticism that I have heard. Thank you very much.

LEHRMAN: In general, I don't want to be misunderstood about what I said on Tocharian. What I said, in fact, was that we don't know enough. I did say, and I want to stress the point, that it is not the language's fault that we have this gaping gap, that really doesn't allow us—I feel—in good conscience to say that Tocharian is in some way archaic. I will refrain from specific citings of archaisms in Tocharian, which are mostly lexical, with the kinds of sound changes in Tocharian. Tocharian really devastated the Indo-European from which it is derived (not Indo-Hittite, but Indo-European) in terms of what it did to the sound system. My point was, let's use what we know and, as the Stoics did, let's know when to abstain. Let us suspend judgment there until we know more and then perhaps, and I hope, we will learn more.

One point on glotto-chronology, the difference between the basic lists of Luwian and Hittite which Professor Ivanov said is 19 percent, which is enough for about a thousand years of separate development. This development is a very standard number that you get in terms of shared innovations. So in no way does it contradict the idea that both of these Anatolian languages came from a common source over the course of about a thousand years, which is what I also—impressionistically, as I admit— suggested in my paper.

A couple of specific things: the terms for *wool* and *wave* and also *spindle*. The Hittite word for spindle, if I remember correctly, is *hulali-*, derived from the same root with the agents, we could say agent-suffix. This root—I believe it exists in the Proto-Indo-European also—has the meaning *to spin*. So we could go a little further and etymologize the root of *wool* as a past passive participle—and it's a recessive formation—of the verb *to spin*. So "the spun stuff," and the spindle would be the spinning thing, as you can see.

IVANOV: Is it not *hulaliya-* that means, just, *to turn*?

LEHRMAN: Well, that's a denominative from *hulali-*. That's a secondary derivative of this noun. But I'm giving you the root and suffix etymology of the word for *wool*. And the word for *wave* may, in fact, be based on the metaphor that ultimately goes back to this *wavy* root of *turning* or *curving, drawing a curve*.

A couple of vocabulary terms that Professor Renfrew mentioned. I don't believe we have the words for *goat, wheat* and *flax* on the Proto-Indo-Hittite level. We do have *barley* and we have *bovines,* very clearly, but no *flax*, no *wheat* and no *goat*. Of course, those animals may have been very different. And it's a very far cry from saying that certain parts of the human body did not exist seven thousand years ago. But what it does show is that that shared experience of these items was not there at that time, and we can't say anything else until we discover them, attested.

And my last little point is the word for *wheel.* I concur that we don't have in Proto-Anatolian the same reduplicative $k^{w}\acute{e}k^{w}los$ for the word for *wheel,* but we do have something that is a word: it's the Hittite *kula,* which means earring. I can't say I've seen those earrings. The word *ring* presupposes a little wheel of some kind. It's the same root *to be round.* So we may think about what that means for the existence or the nonexistence of the wheel. But we do have words in Russian which mean *wheel: kolesó.* Of course we have a different suffix there. So it's not a word equation per se, that's not reduplicated, but may mean *wheel.* They may well be independent formations because they have different morphology, as Professor Melchert pointed out, which is a very important factor. But it is just something that I'm throwing in to this whole pile of stuff.

And I'll conclude on this. As I recall my Lithuanian, just a few forms like the nominative. If we take the Vedic *vīras* and Lithuanian *výras,* there is a striking coincidence in the nominative singular. But as soon as we go away from nominative singular, we will lose those striking similarities, with the exception of the accusative, perhaps, but that is where it ends. And then we have innovations on the part of the Lithuanian which are demonstrable, but I am not going into the tedious details of that. Thank you very much.

Darden: Just a couple of things. I did do my homework, and I thought I said in my talk *arsenical* bronze, not copper. In terms of the innovations experienced in the Caucasus, the specific complex that Chernykh identifies is the producing—the deliberate production—of bronze alloyed with arsenic; the characteristic two-part mold that was developed in the South Caucasus; and specifically the use of that mold to make shaft-hole axes. And this complex spread across the steppe and into Europe. Actually, in the Corded Ware culture they have the two-part molds and the shaft-hole axes, but they actually used pure copper. Maybe the source of arsenic disappeared for a time.

And for wool, let me add an etymology of *wool.* This word that I gave you for *wool* is very long, actually five consonants long. It doesn't look like a primitive word for a kind of hair. So, if you just take off the end, which is a common suffix, you have the root of a word which in Lithuanian means *to make felt.* This is related, with a slight variation, to a Germanic word, *walken,* which means *to make felt.* And in Old English, *wealcere,* which I presume gives us *Walker,* is a *fuller,* or someone who makes felt. It is related to words in Greek which mean *to press together.* If you take off the laryngeal, you have words that mean *horsetail hair* in Lithuanian and Sanskrit and things you make out of horsetail hair. Take away the *l* and you have the word *otum,* in Sanskrit the infinitive of a word that means either *to weave, interweave, lace, braid,* probably some primitive word which didn't mean specifically to weave on a loom but to interlace in a variety of ways. Whether or not all of those are right, if the first step is right, then the word for wool is derived from the *process* by which you made wool cloth. And the fact that it is related to felt, rather than woven wool, makes more sense of the fact that you had woolly sheep around for a couple of a thousand years before you have evidence of woven wool. Maybe the initial process was for making felt and not making woven wool.

There is, by the way, a general word for grain, **yewos*. It appears in Hittite where, I think, it is written *ewan* . . .

LEHRMAN: It means there *barley*.

DARDEN: Well, but the proto-meaning is probably *grain* because that's what shows up in Indo-European and not the meaning *barley*, but some other grain. And there is a word for sheep, not surprisingly. Those are the points I wanted to make.

LEHRMAN: The word ***to make felt*** is a lengthened grade of the same root ***to turn***, or ***to curve***, or ***to twist***. In Russian, for example, *valiat'* is the lengthened grade of that **wol-* which we all know and love in the famous Swedish car recently bought out by Ford. But Russian is simply the lengthened grade of that. So we cannot say that the primary meaning of this is ***to make felt***. But it's simply a derivative of something that means ***to twist***. That's my thought.

PIERRE MACKAY: It is probably extremely unwise for someone who is neither an archeologist nor a linguist to comment at all on this, but I think it is a tribute to the quality of this session that someone who is as far outside the field as I am can find so much excitement in it.

I noticed that the linguistic part of the discussion tends to avoid onomastics, and I want to just reinforce this with a couple of historical questions. Professor Zimansky suggested a possibility of a sort of linguistic hibernation in the Urartian area. We've got a lovely example of that: from 641 for about three hundred years, on the basis of public attestations, or things that last, you would hardly believe that the Persian language continued to exist. Yet because we have historical manuscripts, we know that by 820 the Abbasid califate was terrified out of its mind by an ethnic revival of Iranian speakers, and one hundred years later we have Persian literature again. This temporary lapse of Persian seems even longer, because Sassanian Pahlevi is pretty much restricted to the manuscript tradition.

Then in an entirely opposite case, we have the fashionable language. If we took away about a third of the monuments of the Seljuks of Rum—Anatolia—we would only have names like *Ala-al- Din Kai Ka'us*, we would have names like *Khusrau*, we would have names like *Kabus*. We might easily come to a conclusion that there was an extensive Persian influence in Anatolia in the high middle ages. There wasn't. It was all Turks. And these were Turks who had *no use* for their own native language because Persian was a much more distinguished language to use. It's much, much later that you get the official use of Turkish.

Lastly, there has been a tendency to doubt huge population migrations, and that's probably a very cautious thing to do. But on this continent we can remember that there's a very sizable population of Athabaskans in northern Arizona. We know nothing of the reasons, so far as I know, why those Athabaskans, without the aid of horses, came storming down from north of Saskatchewan and Alberta, but there they are. There they are. And they have left traces behind them. I am told that the Utes still have traditions that recall this rather dangerous migration. Thank you.

RUTTER: There is another item that, for me anyway, seems of considerable importance, and that is the possibility that Professor Finkelberg is correct and that the language of Minoan-Crete was an Anatolian language. If that is true for the first half, let's say, of the second millennium B.C., there are only a limited number of occasions, it seems to me, when an Anatolian language could have been introduced to Crete, if we look only at the archeology of it all. There are some people, I know, who consider Minoan culture to have been significantly changed around 3000 B.C., at the beginning of the Bronze Age. But for many of us there is no significant change in Minoan culture at all, from the time when the island was colonized in the middle of the seventh millennium down to the time of the mid-second millennium, when we have Linear A as this language that Professor Finkelberg has addressed as a possible Anatolian language. And I guess this question unfortunately, Bill, is going to come around to you, because I'm curious to know how you would deal with the presence there of an Anatolian language—it's not proven, of course; I think that Craig's comments make that clear enough—but it certainly is a very live possibility. How are we going to get an Anatolian language onto Crete in the middle of the seventh millennium if we have the split that you describe? Can't do it? Okay. So if you could make progress on that point, it seems to me, that would be critical: the issue of what's going on on Crete.

And Crete, in any case, is going to be very interesting. If Professor Renfrew is correct and Proto-Indo-Hittite is diffused from the center of Anatolia as early as the seventh millennium, then this one branch that ends up on the island of Crete—if it's there as early as the seventh millennium—goes absolutely nowhere for millennia. We may wonder what would happen to a language if it just sat somewhere for five thousand years. Well, it looks to me like we might have a real live candidate there on the island of Crete.

BARBER: I have several miscellaneous points to make here. First, in regard to this last topic, it is sort of generally received wisdom among Cretan archeologists—archeologists of Minoan Crete—that there *was* a major influx of people from southwest Turkey at about 3000 B.C., beginning the Bronze Age. Easy! [laughter].

About the wool and waves, which I found absolutely fascinating. The moment Professor Ivanov mentioned it, I sat there imagining a piece of fleece, raw wool, that was given to me just a week ago, which has the most beautiful waves in it. But this is characteristic of highly evolved fleece. It is not at all characteristic of wild sheep. You saw my slide, if you can recall it again. On the left was typical modern fleece, and it has those beautiful waves in it (it had been teased apart, and the waves are much more visible when it has not yet been combed out). The kemp on a wild sheep and on that rather mangy-looking Bronze Age type sheep on which we saw the proto-wool, that doesn't have those waves. You have to get a fairly evolved fleece to have those waves. Then they are highly evident.

Next point. When we are talking about people running around spreading metallurgy, we have to remember that copper itself is widespread and can be mined in many places in the world, but the alloying hardeners are very hard to find. And you cannot grow them in your backyard the way you can agricultural products. Unlike most things that were desirable in the Neolithic, with the exception of obsidian, you had to go out and hunt for it.

Next point, one that Jared Diamond made very nicely, is that the Eurasian continent is unique among the habitats of early human beings in that it is oriented east-west, so you have tremendously long ecological zones across which people can spread with great rapidity, once they learn how to exploit them. This is very different from north/south movement, where you are having to go across ecological zones, and your technology may not give you an adequate living as you move north or south.

And now I'm going to throw out one more rabbit between the amassed armies of the Persians and the Scythians here, and we can all watch it run. The first time that I heard the proposal to repeal Grimm's law—that is to say, to reevaluate the position of who changed what, in the series of sound correspondences which are known as Grimm's law—was in a lecture that Paul Hopper gave. And then, of course, Professors Ivanov and Gamkrelidze, at almost exactly the same time, published their hypothesis gleaned from somewhat different data and ideas but coming up with much the same model. As I sat there listening to Hopper's version of how to revise our reconstructions of PIE consonants, the linguist in me was listening to the linguistics, but the archaeologist in me was saying: you know, if, when you have repealed Grimm's law, you begin to think about who had to change, was it the people who are living upon the steppe lands today, that is the Slavs? the people just to the west of them, Germanic peoples? Now, I can only evaluate the six branches that I can control, but Slavic and Germanic did relatively little changing, whereas the Greeks and the Indo-Iranian peoples had to do a lot of changing. The old model suggested, I think, that Sanskrit speakers were the guys who never changed anything. That was taken as the basis, and so the Germanic peoples must have changed, and the Slavic peoples changed, and so forth. But if you look at it this other way, the people who did the most changing were people like the Greeks, who, from my point of view, moved into an area where there was already a high culture. With the Indic people it is the same thing. They moved into an area where there was a high culture, and they went through a great deal of changing. And it's not just Grimm's law. Once you start looking at it this way—who did massive amounts of vocabulary borrowing—apparently the Greeks, the Germanic peoples, on that end, the Iranians, the Indic peoples, did lots and lots of borrowing. So I think this is another thing we should take into account for figuring out where the homeland was: Who had to change? People who don't move far away from home don't have to change much. People who move into a whole new area have to learn a whole new way of earning a living, bump up against cultures that have a lot of really interesting ideas and things you might want to borrow. Those are the guys who are going to do all the linguistic changing, and it's worth thinking about this archeo-linguistically. Thank you.

VICTOR MAIR: I feel like an interloper from the other end of Asia, since I come from the China side, but I want to speak, to give a passionate defense for Tocharian, which is out in my neck of the woods. Professor Lehrman takes an extreme position regarding the value of written records. He accepts the importance of Indo-Iranian, which he puts at around 1000 B.C., but I would like to point out [that] the actual manuscripts for both Sanskrit and Avestan are much, much later, not anywhere near 1000 B.C. His argument is that Tocharian manuscripts date to the seventh and ninth century of our era; they are not very old, therefore not very relevant. He adopts a very dismissive tone. He says Tocharian doesn't really matter. Or as he said just now in his more moderate remarks, let us abstain from thinking about Tocharian because we don't have enough of it.

In my brief defense of Tocharian I will invoke only the linguistic evidence, not historical or archeological evidence, for which there isn't time. One, we cannot ignore the archaic features of Tocharian. Some of these it shares with Hittite, and there are early innovations with Luwian, which Professor Ivanov just showed us, and I won't reiterate those. It does have these features.

Two. We can't ignore features of Tocharian that it shares with other Indo-European groups. We have enough evidence to make these connections, as shown by Adams and Ringe, and in a very magisterial article by Hamp recently.

Three. Everyone would agree that Tocharian is an Indo-European language; I don't think anyone would deny that. We have to take it seriously as an Indo-European language. Even though its geographical position and relatively late date may be challenging. We take Baltic seriously even though written records are later than those for Tocharian. In fact, I would argue that precisely because of its special position, its geographical location and its complex of linguistic features, Tocharian is absolutely crucial for understanding the history of Indo-European.

Tocharian A and B: we have at least two dialects. This presumes that there was a Proto-Tocharian, and people usually say that Proto-Tocharian would have to be at least five centuries earlier than Tocharian A and B. You also have Tocharian C, which is embedded in Kharaṣṭhī Prakrit, which we have from the third century of our era; so that takes us back pretty far. Then we can go back a lot further in time with borrowings into Chinese. That's why I stand here as a China person to tell you that we do have some relevant borrowings from Tocharian into Chinese, such as words for *lion* and *honey.* And these go back to the second century B.C., But we can go back still further with Chinese evidence and Sasha Lubotsky, although I think he took it too far, was looking at dozens of words just related to the chariot and to wall-building terms in sixth-century B.C. Chinese poems.

Then we need to go back and look at oracle bones from 1200 B.C. I have to stress that all of this is just in its infancy, because people, too long, have been putting China off at the other end of Asia and just not paying any attention to it, thinking that it is separated off behind a great wall and has no relevance to what we are doing in Indo-European or anything else in the world. Well, it does. As we join Chinese studies with studies around the world, you will be seeing Tocharian words. I connected *Maguš*, which is an Iranian word, with *Maguš* that is found on an oracle bone from 1200 B.C. So Indo-Europeans were out there. If we have gotten back to 1200 B.C., we're not that far from 2000 B.C., which is the magic number for Hittite. So I think we're old enough! [laughter].

And furthermore, we are finding more materials all the time, but recently Ji Xianlin, Werner Winter and Georges-Jean Pinault published a marvelous new text of the Tocharian A *Maitreyasamiti-Nāṭaka*, a huge expansion of the amount of material we have for Tocharian, all in one leap. And I guarantee that we will find more.

And now I will just close very briefly with this. I was very happy to hear what Professor MacKay said about migrations. Professor Renfrew yesterday said Minoan came to Crete with the first farmers. Okay. And Indic seems to have come with somebody to south Asia. And Tocharian seems to come with somebody to eastern-central Asia. My question to all of you, linguists and archeologists, is: Can languages move without bodies? Now, if they can move without bodies, without people, I would like to know how this process works: a language transferred without people moving. If the movement of people is normally necessary for the transfer of languages, then don't we have to take migration seriously?

DREWS: Thank you, a good question on which to end. I wish that we could go on, but it's now twelve o'clock. If this conference has been as stimulating for all of you as for me—and I sense that it has—then I hope we will continue what we started here.

GREATER ANATOLIA

PROTO-ANATOLIAN, PROTO-INDO-HITTITE, AND BEYOND

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Our colloquium served more to highlight differences of opinion than to resolve them. A few pieces of common ground were reached, and they are noted below, but common ground is not the only gain from a colloquium. The exchange of ideas, and sometimes broad disagreements, demonstrated how pliant is the evidence on which various favored interpretations rest. In the give-and-take, proposals and arguments were advanced that look very promising for the future. And some problems were exposed, which need to be addressed.

One of the most important observations made at the colloquium, and the one with which it is appropriate to begin this retrospect, is that the Indo-Hittite theory is either rapidly becoming (Lehrman) or already is (Melchert) the framework within which most Indo-Europeanists work. Most recently, Alexander Lehrman's own work and the cladistic analysis by Tandy Warnow, Donald Ringe, and Ann Taylor have contributed to the respectability of the Indo-Hittite theory,[1] which had begun to emerge from the shadows by the end of the 1980s. In Craig Melchert's words (p. 233), "The crucial point is that limited but compelling evidence now shows that the rest of the Indo-European languages underwent a set of shared common innovations in which Anatolian did not share." For most of us nonlinguists the agreement on this basic point was a surprise and created some excitement. From my standpoint as organizer of the colloquium, whatever else we nonlinguists learned or unlearned, that new perspective itself repaid the bother of organization. The rapidly growing support for the Indo-Hittite theory among linguists is obviously a development of considerable significance for historians, prehistorians and archaeologists.

It is nevertheless true, as was remarked more than once and as was immediately evident from the papers themselves, that the Indo-Hittite theory means rather different things to different linguists. Some scholars who have embraced the substance of the theory are reluctant to use the name *Indo-Hittite* (almost everyone, it may here be noted, concedes that the name is unfortunate), and vice versa. As Bill Darden articulates the alternatives, the weakest version of the Indo-Hittite theory concedes little more than that Proto-Anatolian (PA) was the first language branch to diverge from Proto-Indo-European (PIE) and, so, is "older" than Proto-Greek, Proto-Tocharian, and the rest. In the first divergence of linguistic branches or streams, that is, a single *Ursprache* diverged into two branches, one of which was the Proto-Indo-European branch and the other the branch that would become Proto-Anatolian, and for the *Ursprache* the name "Proto-Indo-Hittite" (PIH) is gaining acceptance. But according to this weak version of the theory, the so-named PIH was not very different from the PIE that was soon to diverge into the various Indo-European sub-groups. In this view the PA branch innovated extensively, especially with "negative innovations" or loss of PIH features. Finally, in the weak version, the bifurcation of PIH into PIE and PA is often supposed to have occurred not too long—perhaps not more than a few centuries—before the subsequent branching of PIE into the various historical Indo-European sub-groups.

The strong version of the Indo-Hittite theory, contrarily, assumes that a very long time indeed (closer to a few millennia than to a few centuries) elapsed between, on the one hand, the initial division of PIH into branches *that would become* PIE and PA and, on the other hand, the secondary branching of PIE into Proto-Tocharian, Proto-Aryan, Proto-Keltic, and the rest. And during this long time the language that would become PIE underwent many and radical "positive" innovations, which transformed it into something very different from PIH. Adherents of this strong version of the theory reserve the term *PIE* for the language in use *after* the assumed innovations had taken place and immediately *before* the secondary branching began. A corollary of the theory is that the branch of PIH that culminated in PA was less innovative, with the result that PA bore more resemblance to PIH than did PIE.

Neither the strong nor the weak version of the Indo-Hittite theory quite fits with Vyacheslav Ivanov's view, as expressed in this volume or as elaborated with T. Gamkrelidze in 1985. Ivanov here argues that South Anatolian (Luwian) and North Anatolian (Hittite) were each an independent offshoot of what he prefers to call Proto-Indo-European, and thus he obviates the need for any "Proto-Anatolian" intermediary. In his view (see his concise statement of it on p. 148), the PA that is conventionally postulated was essentially identical with a very early stage of PIE. However, because Ivanov does see his two Anatolian languages as older than all other languages that branched from his PIE, he does accept a salient tenet of the Indo-Hittite theory.

Terminology

With this latitude among linguists in their use of the terms *PIE* and *PIH*, it is no surprise that among archaeologists and historians the terms are sometimes used in an almost arbitrary way. I was guilty of that in my *Coming of the Greeks*, and Colin Renfrew has stretched the term *PIE* to cover, in various phases, five thousand years of linguistic development. Elizabeth Barber, aware that "Proto-Indo-European" means vastly different things to different participants and registrants, with deliberate whimsy coins her own term, *proto-Us*, in an attempt to open the way for new perspectives. It became evident as the colloquium progressed that it is now more important than ever to be careful about our terms: we have reached a point at which some agreement on terminology is essential if discourse is to be productive.

Since the Indo-Hittite theory is becoming, or has already become, widely accepted, whether in a weak or a strong version, we have the opportunity—and should seize it—of restoring to the term *Indo-European* its pre-Hrozný meaning, when it stood for Sanskrit, Greek, Latin, and other kindred languages known in the nineteenth century but not for Hittite, Luwian, and the rest of the Anatolian languages. We have the luxury of tightening the definition of *Indo-European*, because the label "Indo-Hittite"—however *mal trovato*—is now available for languages that do not qualify for the inner circle. Carian specialists, for example, might now readily concede that Carian was not an Indo-European language and concentrate instead on showing its Indo-Hittite qualifications. The debate about Tocharian could also be facilitated if, while unanimous that Tocharian is Indo-Hittite, debaters narrowed the question to how well Tocharian fits, let us say, the Indo-European that Karl Brugmann described in his *Kurze vergleichende Grammatik der indogermanischen Sprachen* (Strassburg, 1903). If, ultimately, a consensus forms that Tocharian cannot be aligned with the eight or nine subfamilies that Brugmann and his contemporaries recognized as Indo-European, efforts to make Tocharian conform to PIE and vice versa could be called off, and energies could be re-directed to an exploration of what Tocharian reveals about the immediate prehistory of PIE. As for the latter term, if it were agreed to classify as "Indo-European" only the Brugmannian sub-families, then it would follow logically that *PIE*, as Darden urged in our colloquium, must denote the language in use *immediately before* the first of these eight or nine subfamilies branched from the parent stem. Whether that first branching led to Proto-Aryan, Proto-Keltic and Italic, Proto-Greek, or proto–something else will, of course, be debated, but in principle the definition should hold. That, at any rate, is how the terms *IE* and *PIE* will be used in the rest of this essay.

We are required to tidy up the definition of IE and PIE and can afford to do so, because it has now become obvious that PIE was only one stage, although one of extraordinary interest, in a continuous linguistic development. In the nineteenth century many philologists and historians had a tendency to assume that PIE was a kind of linguistic bedrock. To trace the historical IE languages back to PIE was to trace them back to the beginning, or back to a stage beyond which it was impossible to see. And the languages of history fell into two sharply demarcated categories: one was IE, and the other was not. The Indo-Hittite theory, on the other hand, declares that PIE itself had a prehistory, the last part of which is to a considerable extent recoverable, and recognizes that the IE languages were related, more or less closely, to quite a few other languages.

Strictly speaking, the "Anatolian" languages in this essay are Hittite, Luwian, and Palaic, all attested in the Hatttusas tablets and (pace Ivanov) regarded by most

Hittitologists as cognate. To those three we must add Lycian as a fourth "Anatolian" language. Proto-Anatolian is necessarily the hypothetical language from which these four historical languages were descended, and which was spoken immediately before the earliest branching that eventually led to one of the four historical languages. Whether other languages spoken in western Anatolia belong in the "Anatolian" group is a very important question, the answer to which is not yet clear.

The terms *Indo-Hittite* and *Proto-Indo-Hittite* also have fairly precise definitions in what follows here. In whatever version of it one is thinking, *Indo-Hittite* is the name for a language family that is somewhat larger than the Indo-European family, since it includes all of the latter plus the four Anatolian languages. The hypothetical language from which this larger family of languages came is Proto-Indo-Hittite. Analogous to Darden's strictures about PIE, the term *PIH* should be reserved for the language that was spoken *immediately before* its divergence into two branches, the one leading to the Anatolian languages, and the other to what we must now call the Indo-European languages. It is not out of the question that PIH gave rise to still other branches. Is it possible, for example, that Lydian may have descended not from PA (and certainly not from PIE) but from a third branch of PIH?

At present, PIH seems to be as far back into the prehistory of the Indo-European and the Anatolian languages as anyone is able to see. It is conceivable, however, that some day a still earlier stage will become visible, as specialists concur that PIH itself was cognate with still other languages or language families. Everyone will admit, surely, that PIH was no more an *Ursprache* than was PIE and that PIH too had a prehistory, stretching back ultimately to the origins of language itself. What remains to be seen is whether the latter part of that prehistory will one day appear, however dimly, on what Renfrew has called "the edge of knowability."[2] Should that happen, yet another term will be required to denote that still larger family of languages, and a new name for the proto-language from which PIH and its yet-to-be-discovered siblings came. In this essay I shall use the name "Pre-PIH" to refer to this hypothetical language, ancestral to PIH. Surely a better name than that can and will be coined when the need arises, but for purposes of this essay *Pre-PIH* will do. Needless to say, if years from now linguists do begin to discuss Pre-PIH, their discussions will depend as much on conjecture as on science. For even the most assiduous investigator, so remote a language as Pre-PIH—in use long before PIH came to maturity—will necessarily be far less distinct than PIH and by comparison will make PIE seem to have been almost completely reconstructed.

Proto-Anatolian and the Languages of Anatolia

Several presenters—Barber, Darden, Ivanov, and Renfrew—explicitly or implicitly suggest where and when PIE was spoken, and I shall return to those suggestions in due course. But attention focused especially on PIH and its PA branch. On the question of PA, a few small but important pieces of common ground have been reached.

Our linguists seem to be roughly in agreement that the Anatolian languages attested for the Late Bronze Age (1650–1200 B.C.)—Hittite, Luwian (in both its cuneiform and its hieroglyphic dialects), and Palaic—were no more akin to each other than, for example, were Irish and Welsh in the early modern period, or than the Romance languages are today. Proto-Lycian, for which we have no early documentation but which obviously existed in the Bronze Age, may still have been fairly close to Luwian in the early second millennium but must have already been a distinct language. Ivanov argues that the differences between Southern Anatolian (Luwian and Lycian) and Northern Anatolian (Hittite and Palaic) are striking enough that the similarities in the two Anatolian "zones" should be explained as the result of convergence between two originally disparate language communities, rather than as the result of a diverging descent of cognate languages from a common original. The very useful tables (1a, 1b, 2, and 3) that Ivanov has appended to his paper will allow readers to see for themselves how many Luwian and Hittite words for a basic vocabulary item came from the same root, and how many came from different roots.

Although Ivanov's radical thesis is rejected by Lehrman and Melchert, it obviously buttresses their conclusion that the Anatolian languages had been going their separate ways for a long time prior to the first written evidence for these languages. Specifically, both Lehrman and Melchert are comfortable with the likelihood that PA began to splinter into the historical Anatolian languages as early as ca. 3000 B.C. If one were to define the Anatolian branch widely enough to include Lydian and Carian, an even earlier date for PA would undoubtedly be preferred. A small but solid point relevant to the dating of PA is Peter Kuniholm's observation (p. 32) that the date for Kültepe Level II, the earlier level of the Kanesh *kārum*, will have to be pushed back by about sixty years. The Old Assyrian tablets found in this level include many names that seem to be Hittite and others that seem to be Luwian. Kuniholm's dendrochronology, and the extended *limmu*-list about to be published by Klaas Veenhof, would now put the first of those names a bit earlier than 2000 B.C.

That Hittite and Luwian were apparently differentiated already by 2000 B.C., and that PA may have been spoken no later than ca. 3000 B.C., should be of interest to archaeologists, pre-historians, and the few of us historians who worry about the Bronze Age. To put this in proper perspective, it may be helpful to recall how things looked sixty years ago, as the Golden Age of nationalism was coming to a close. At that time most scholars still accepted the nineteenth-century assumption that language was (or ought to be) an expression of nationality: *quot linguae, tot nationes*. Before the Second World War, then, it was widely assumed that ca. 1900 B.C. "the Luwians," "the Hittites," and "the Palaites" were evicted from their homelands north of the Black Sea. Each of the three groups was supposed to have been an independent nation, although kindred to the other two, as similarities in the three languages indicated. Eventually the kindred nations invaded Anatolia, where each of them carved out for itself a new homeland. As late as the 1960s a well-informed archaeologist could believe that "at the beginning of the 2nd millennium B.C. Asia Minor was invaded by successive waves of tribes belonging to different branches of the Indo-European stock."[3] As Renfrew notes, for a long time the question of state-formation in central Anatolia was neglected, because it was assumed that "the arrival of the Hittites" from north of the Black Sea offered a sufficient explanation for Labarnas's establishment of a Great Kingdom at Hattusas.

After the Second World War, however, the general understanding of language differentiation began to be modified by *Sprachgeographie*. Linguistic differences, that is, were now explicitly described as consequences of physical or political geography, rather than being implicitly understood as expressions of national identities: what began as a single ancient language might have diverged into two or more dialects and finally into two or more distinct languages because of the geographical extent of the area in which it was spoken, because of physical features such as a mountain range or a wide river, or because of economic or political attractions and constraints. These principles of linguistic geography have changed how one discusses the origins of the Anatolian languages, indeed, of all languages. Whereas earlier it was possible to assume that Hittite, Luwian, and Palaic originated because a single "ethnic stock" divided into three nations somewhere north of Anatolia, and that the languages were brought to Anatolia by folk migrations, it is now admitted all round that because of various factors of physical, political, or economic geography a single Proto-Anatolian language diverged into three or more languages. Whether this happened inside or outside Anatolia is still debated, but at least general agreement has been reached about the process through which one language becomes several languages. In the process as presently conceived, there is no need for nations. Instead of reckoning with such entities as "the Luwians" and "the Hittites," one can reckon with people who spoke Luwian, other people who spoke Hittite, and still others who spoke both of those languages and possibly more besides.[4]

If it is conceded, contra Ivanov, that the languages of the Anatolian language family evolved from Proto-Anatolian, we are left to debate where and how this differentiation occurred. On the one hand, Darden and Barber assume that it happened north of the Black Sea and propose that Luwians and Hittites (and, perhaps, Palaites, although they are not mentioned) came into Anatolia by different routes and at different times.[5] On the other hand, Margalit Finkelberg, Lehrman and Renfrew assume that the differentiation of the Anatolian languages from PA occurred in Anatolia itself. They assume, that is, that various factors encouraged the divergence of PA first into dialects and finally into the historical languages in the very places where these historical languages were in fact spoken. Because this assumption has nowhere been elaborated, I shall here try to fill that gap. The particulars presented in these pages are not new. But since, so far as I know, they have not yet been brought together and deployed in the service of *Sprachgeographie*, this attempt at a synthesis should be of some value. I will begin by reviewing the roster of languages that were either in the Anatolian family or related to it.

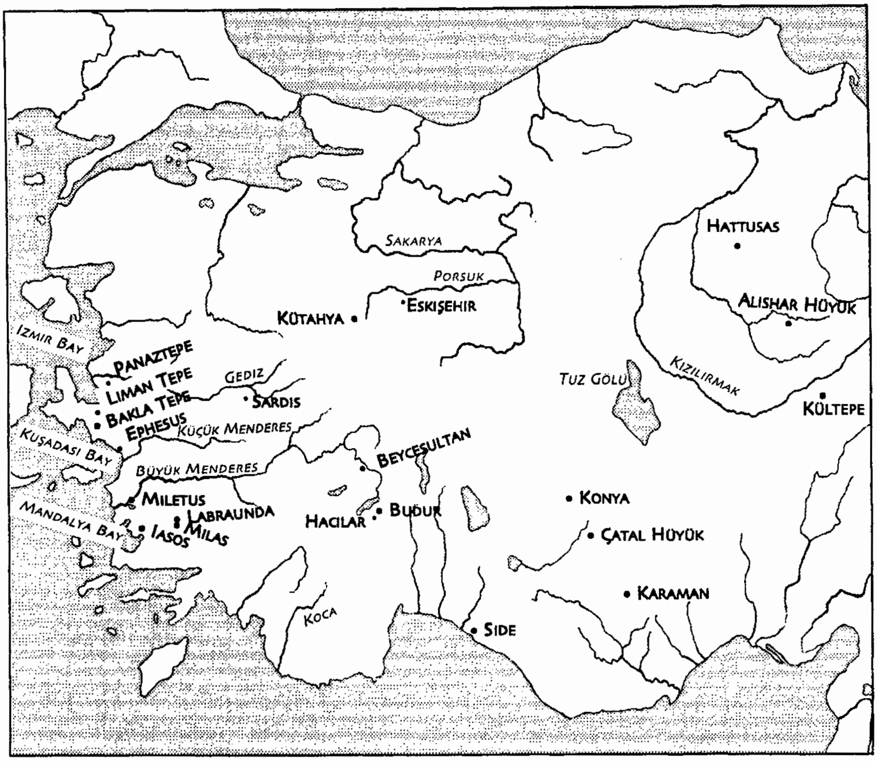
CARIAN AND LYDIAN

Carian and Lydian made only cameo appearances at our colloquium. It is noteworthy, however, that those of our linguists who referred to them seem agreed that these two languages were descended from (or possibly cognate to) PA. This is explicitly stated by Finkelberg (p. 96), Melchert (pp. 231–32) and Lehrman (p. 114), and although Ivanov mentions Carian only in passing, he assumes the "Anatolian" character of Lydian, placing it in his "northern Anatolian" zone (see, for example, his p. 163n.1). Our linguists' agreement on this point can be contrasted with the prevailing view a hundred years ago, when most scholars accepted Paul Kretschmer's conclusion that Lydian, Carian, and even Lycian had no connection with the Indo-European languages.

The language community that came to be known as Lydia was a riverine district, no more than eighty miles from north to south, in the center of Anatolia's Aegean coast. In Herodotus's time, "Lydia" was bordered on the north by "Mysia," which must have been another language community and which in the Bronze Age may have reached not only to the coast and the island of Lesbos but across the Dardanelles and Bosporus into Thrace.[6] But of the Mysian language there is nothing left. The Lydian-speaking community in the Bronze Age probably spilled over into the large offshore islands of Chios and Samos, which lie within sight of the mainland and which seem to have been outside the "Greek" orbit until after the destructions that ended the Bronze Age.[7] On the islands no Neolithic settlement has yet been found, and on the mainland Neolithic evidence is very limited. But in the Bronze Age the Lydian coast flourished, with "sophisticated cultures" appearing in the third and second millennium.[8]

In the Early Bronze Age (EB) by far the largest concentration of people seems to have been along the most northerly of Lydia's rivers, the Gediz (Hermos), and around the Izmir bay, into which the Gediz flows (see map 1). All through the Gediz valley, where Magnesia and Sardis stood in the Iron Age, evidence for EB habitation has been found.[9] Up and down the coast from the river's mouth were more EB settlements, and a town distinguished by a "corridor house" stood at Liman Tepe, on the southern shore of the Izmir bay. (In the first millennium B.C. this was the site of Klazomenai.) Across the bay, at Panaztepe, was another large site that was continuously occupied from the third millennium to the Iron Age.[10] Forty airline miles to the south, on the Kuşadasi bay, an EB town is being dug at Bakla Tepe, north of the mouth of the Küçük Menderes (Kayster) river and of Ephesus (Apasa in the Bronze Age). The island of Samos, where a significant EB settlement has also been found, was almost an extension of the Kuşadasi bay's southern arm. Still further to the south was the delta of the Büyük Menderes (Maiandros, or Maeander) river. In classical antiquity, before millennia of silting extended Anatolia into the Aegean, Miletos and old Priene overlooked this bay. In the second millennium B.C. Miletos was evidently the place known to Hittite kings as Millawanda and was already an important city. Probably controlled for a time by the "Minoan" king of Knossos, it was soon taken

Map 1



over by a Mycenaean ruler and later destroyed by a Hittite king.[11] In the Early Bronze period Miletos was evidently just a small town. Up the valley of the Büyük Menderes evidence for EB settlement is relatively thin, suggesting that in the third millennium considerably fewer people lived here than lived along the Gediz and on the Izmir bay. It must be remembered, however, that Beycesultan is located at the headwaters of the Maeander.[12]

We can presume that the language spoken on the Aegean coast of central Anatolia in the Bronze Age, and very likely on Samos and Chios and some of the smaller offshore islands, was Lydian or Proto-Lydian. An exception would perhaps have been Miletos, where at least some people must have spoken "Minoan" in the second quarter of the second millennium (much "Minoan" material has been found there, as well as a Linear A inscription) and South Greek in the Mycenaean period. But Miletos may have been a special case, since its material culture is quite different from that found in other towns on the Lydian coast. Looking to the interior, we must note that between the putative Lydian-speaking coastal zone and the thickly settled plateau (where, as we shall see, the language was almost certainly Luwian), there intervened more than a hundred airline miles of rough country, which in the third millennium B.C. was either sparsely settled or entirely uninhabited. This wilderness stretched from the upper Gediz valley eastward almost to present-day Kütahya and Afyon.

The upheaval ca. 1200 B.C. that ended the Bronze Age seems also to have ended the Lydian communities on the Aegean coast and the offshore islands. Small coastal settlements, unless very well fortified, were especially vulnerable to raiders who came by ship, and apparently the wiser counsel was to abandon the coasts and to find safer locations inland. After a short interlude but probably before 1000 B.C., refugees from the Greek mainland sailed to the islands and the coast of Anatolia, where they built or rebuilt a line of cities, presumably well walled. In these cities the dominant language was Ionic or Aeolic Greek, but evidently those Lydian-speaking natives still remaining in the coastal plains collaborated in the creation or reconstruction of such cities as Miletos, Ephesos, Kolophon, Klazomenai, and Kyme. It was surely from natives of Lydia, for example, that the Greek speakers of Ephesos learned to worship Artemis, the city's main deity until its Christianization.[13] In the sixth century B.C. Lydian was hardly spoken on the coast, which had become "Ionia." (North of the Hermos, coastal Mysia likewise became "Aeolis.") By default Iron Age "Lydia" was the interior of western Anatolia, or the land east of Ionia. But in the Bronze Age the Anatolian coast was not yet Greek speaking, and here is where Lydian must have been most in evidence. I would guess that in the Late Bronze Age at least a hundred thousand people must have spoken Lydian.

It is therefore paradoxical that less is known about the Lydian language than about Carian, the evidence for Lydian being slightly more than a hundred inscriptions and graffiti on clay or stone.[14] An *Indogermanisch* affiliation for Lydian was proposed by P. Meriggi (Meriggi 1936). Subsequent studies by A. Heubeck and R. Gusmani discovered features of Lydian that could be paralleled with Luwian and Hittite, although much remains uncertain. Personal and relative pronouns are the most telling evidence that Lydian belongs to the Indo-Hittite family. Olivier Masson cautioned, however, that "the phonetics of Lydian still present difficulties and little is known of the vocabulary, with the result that it is impossible to make coherent translations or to provide a detailed table of correspondences."[15] Masson did note that the Lydian words for *house*, *boundary*, and *god* seem to come from the same roots as the equivalent Hittite words.

Although specialists fifty years ago admitted Lydian to what they called the "Indo-European" family, it is only in the past few years that Carian's credentials have been approved. As late as 1980 few specialists believed that Carian had any connection with "the Hittites" and "the Luwians." This scholarly chapter seems now to be closing. Although the matter is not yet quite settled, the world's very few Cariologists are concluding that in one way or another Carian was indeed related to Hittite, Luwian, and the other Anatolian languages.

Alphabetic inscriptions, dating from the third quarter of the first millennium B.C., were found in Caria proper already in the nineteenth century. But the letters certainly did not have the same values they had in the Phoenician or Greek or any other known alphabet. (It turns out, for example, that in Carian the vowel sound "u" was expressed with four different letters). So long as no agreement could be reached about the decipherment of the script, neither could there be agreement about the classification of the Carian language. As recently as ten years ago Masson summarized the situation as follows:

> Since the script has not yet been completely deciphered, the Carian language itself remains an enigma. In theory there are two possible solutions: either Carian, unlike Lydian and Lycian, is truly an "Asianic" language, relatively autochthonous and not Indo-European, or else it is an ancient Anatolian language of Indo-European origin, like the languages mentioned earlier.[16]

The second solution, if we substitute "Indo-Hittite" for "Indo-European," is now in the ascendancy, thanks to unexpected illumination from Egypt. Some fifty funerary inscriptions, many of them Carian-Egyptian bilinguals, were found at Saqqara in 1968. The bilinguals helped John Ray to propose a decipherment,[17] which other specialists—especially I.-J. Adiego and Diether Schürr—adopted and fine-tuned. Confirmation came in 1996, when Turkish excavations at Caunus unearthed a marble stele inscribed in Greek and Carian. The Caunus bilingual, according to Melchert, "has amply confirmed the essential validity" of Ray's decipherment,[18] and today that particular problem can be considered solved.

Understanding of that language itself has not moved ahead so dramatically, but the decipherment of the script is leading to a broad agreement that Carian was related to the Anatolian language family. Ray himself decided that the Carian language was Indo-Hittite ("Indo-European," in his terminology),[19] and most other specialists agree. In 1995 Melchert stated that "we can already affirm that Pisidian, Sidetic and Carian are Indo-European Anatolian languages, on the strength of one highly characteristic construction: the use of the relational adjective to mark possession cited above for Luwian."[20] To capitalize on the gains that had already been made, a Carian colloquium was held near Zurich in the fall of 1997, and the papers appeared in volume 37 (1998) of *Kadmos*. The verdict that Carian either belongs to or is closely related to the Anatolian subfamily is not unanimous. Tempering the optimism at the Zurich colloquium, Carruba expressed grave reservations not only about the affiliation of Carian to the Anatolian language family but even about the decipherment. Specialists who expected that Carian would turn out to be closely related to Luwian, Carruba argued, were wrong: "In Wirklichkeit lassen die bisher vorgelegten, einander widersprechenden Konsonanten- und Vokalhäufungen kein luwisches, ja sogar kaum anatolisches Lexikon zum Vorschein kommen." But that was a minority opinion, which Carruba himself had modified by the end of the

Zurich colloquium.[21] In a 1999 article Theo Van den Hout asserts that the identification of *sb* as the Carian conjunction *and* (made independently by three Cariologists in the early 1990s) "virtually proves the Indo-European-Anatolian character of the Carian language."[22]

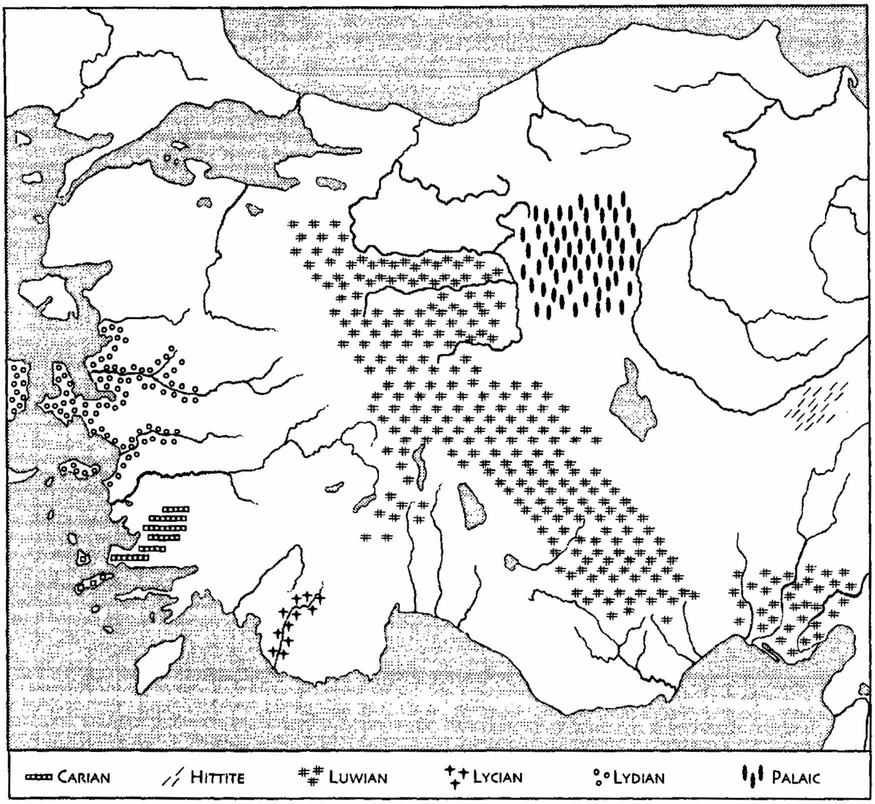
What was Caria? Like Lydia and Mysia, it was a language community ("barbarophone Carians," Homer called its members) on Anatolia's Aegean coast, but it was more restricted than Lydia. Although by the fifth century B.C. Carian speakers could evidently be found in the mountains just to the south of the Maeander, it is likely that in the EB the Lydian and Carian language communities would have been separated by many miles of sea or wilderness. On a typical map of ancient Anatolia, Caria appears to cover all of the southwest corner of Anatolia, an area of about 10,000 square miles. But that is misleading. Most of that large area is mountainous, and the original Caria seems to have been a small but fertile plain that came down to the Mandalya bay (see map 2). The plain began narrowly at the coast, near classical Iasos, and broadened as it extended to Mylasa, some fifteen miles inland. This plain, an area of about three hundred square miles, was blest with good soil and enjoyed the fine Mediterranean climate of Mandalya bay. Similarities in material culture suggest that in the third millennium B.C. the inhabitants of the Mylasa plain were in close contact with people on the larger of the Dodecanese islands, such as Leros, Patmos, Kalymnos, and especially Kos.[23] Herodotus (1.171.2) records the Cretans' belief that "the Carians" were originally islanders called *Leleges*, and had served as sailors under King Minos. Whatever Carian-speaking communities there were in the Dodecanese were destroyed or abandoned at the end of the Bronze Age, and in the Iron Age Carian was heard only on the mainland, and especially in its interior.

In the fifth century B.C., by which time most inhabitants of the coastal cities spoke Greek and (like Herodotus of Halikarnassos) thought of themselves as Greek, the most important "Carian" city was Mylasa. According to Strabo 14.2.23, a road led from Mylasa to Alabanda, across the mountains to the north, and for the first sixty stades (seven miles) out of Mylasa the road was a Sacred Way: at sixty stades the road reached the mountain pass, where there was a village and "high place" called Labraunda, perhaps the most important of the Carians' cult centers. The Mylasa plain, then, close to the Aegean coast and in touch with the offshore islands called the Dodecanese, constituted the essence of Caria. The rest of what by the fifth century B.C. had become Caria was mountainous, and its climate was less welcoming. As the Carian language, over the millennia, spread through these mountains, and as coastal Caria gradually became Hellenized, "Carians" came to be identified with little more than the wilds. Archaic Greek poets thought of "the Lydians" as urbane and of "the Carians" as uncouth, well suited for the life of a mercenary soldier.

How far back the antecedents of Carian and Lydian went in their historical homes is difficult to estimate. For what it is worth, in conversation with classical Greeks the people who identified themselves as Lydians, Carians, and Mysians

Map 2.

Suggested locations ca. 2000 B.C. of languages ancestral to Carian, Hittite, Luwian, Lycian, Lydian and Palaic.



believed that their three nations were aboriginal, being descended from Lydus, Car, and Mysus, the three eponymous sons of the divine Atys (Attis). According to Herodotus (1.171.5), the Carians claimed that "they are autochthonous mainlanders." Archaeologists have not been able to confirm or disprove the claim. As summarized by E. M. Melas, "Caria—despite several archaeological reconnaissances—seems to be almost totally barren of pre-EBA remains."[24] Since Neolithic settlements in Caria have not yet been found, it is possible that the roots of Carian were not planted in the Mylasa plain until after 3000 B.C., Lehrman's and Melchert's notional date for PA (which is to say, the date for the divergence of the traditions that were to culminate in Luwian, Palaic, and Hittite).

But we should not exclude the possibility that the antecedents of Carian and Lydian (and presumably of Mysian as well) came to Anatolia's Aegean coastlands long before 3000 B.C., and long before the divergence of Luwian and Hittite began. Neolithic settlements are generally more difficult to find than Bronze Age settlements, in part because until bronze tools came into use the art of cutting wood and stone was so primitive that "Neolithic architecture" is something of an oxymoron. By way of comparison, except for Thessaly the Greek mainland during the last millennium of the Neolithic period has produced so little evidence of human settlements that some archaeologists have suggested that it was virtually uninhabited. Neolithic remains in Egypt, Palestine, Syria, and the Anatolian plateau itself are likewise far scantier than EB remains.

That Neolithic Caria was uninhabited is quite incredible. Hacilar, directly east of Caria, was a Neolithic settlement already in 8000 B.C. The colonizing of Crete, no later than ca. 6500 B.C., presumably proceeded through the Dodecanese to Carpathos and the smaller islands that lie like stepping stones between southwestern Anatolia and Crete.[25] For such a colonizing expedition, the Mandalya bay would have been the most natural point of departure from the mainland. With the Bronze Age, settlements began to appear in Caria. The *Tübinger Atlas des Vorderen Orients* shows that evidence for EB settlements in the entire area from the Büyük Menderes southward amounts to scarcely 10 percent of the quantity of evidence for an area of comparable size on the plateau; but there are a few EB sites exactly where one would expect to find them, in the plain between Iasos and Mylasa.[26] The argument from place-names also suggests that Caria was occupied in the Neolithic period. As is well known, scores of place-names in western Anatolia and Greece end in the suffixes *-ssa* or *-nda* (the obligatory addition of a *sigma* for nominal endings in Greek normally changed these suffixes to *-ssos* and *-nthos* in Greek-speaking areas), and archaeology has shown that some of these places were established very early in the Neolithic period. Place-names in Caria have the same suffixes: Iasos, Mylasa and Halikarnassos belong to the first category, and in the second are Alabanda, Labraunda, and—a name perhaps related to *Caria* itself—Caryanda. In short, the population of Neolithic Caria may have been very small, perhaps no more than a few thousand people, but from the circumstantial evidence we should be safe in inferring that the Mylasa plain was inhabited in the Neolithic period.

Unless evidence to the contrary is discovered, we should also assume that the first settlers on Anatolia's Aegean coast spoke languages that became, respectively, Lydian and Carian. We have seen that in Luwian and Hittite, according to Ivanov's tables, approximately 80 percent of the words in basic vocabulary lists came from a common PA root. Because I know neither Carian nor Lydian, nor any other Anatolian language, I do not know how Carian and Lydian would compare with the languages known from cuneiform tablets. From the candid comments of specialists, however, I suppose that neither would do nearly so well as either the cuneiform languages or Lycian.[27] Lycian may be especially relevant, since it was undoubtedly descended from PA and, like Carian and Lydian, is attested only from the first millennium B.C. If the percentage of the known Lydian and Carian vocabulary that can be traced to PA is much smaller than the percentage of PA roots in Lycian, the implication should be that the parent tongues of Lydian and Carian had diverged from PIH or from Pre-PIH a long time before PA completed its evolution.

In traditional Indo-European scholarship, if Carian had any connection at all with "the Anatolian branch of Indo-European" it had to be made a descendant of PA. The theory holds that there is no other place for Carian and no other explanation for its presence in southwestern Anatolia. In the Indo-Hittite theory, on the other hand, Carian could conceivably have been related to the Anatolian group (i.e., to Luwian, Lycian, Palaic, and Hittite) but may nevertheless not have belonged to the group: it could have been descended from an Indo-Hittite language cognate to PA, or even from a language cognate to PIH. The point is important enough to deserve rephrasing and repeating: We should admit, as a theoretical possibility, that Carian was not, strictly speaking, an "Anatolian" language and possibly was not even an Indo-Hittite language. Although undoubtedly related to the cuneiform Anatolian languages, Carian may have belonged not to the Anatolian (and certainly not to the Indo-European) branch of PIH, but to a collateral third branch. And it is even conceivable that it goes back still further on the evolutionary tree, coming from a linguistic tradition that issued from Pre-PIH, well before the maturing of PIH and its branching into the traditions that were to become PIE and PA.

Although the roots of Lydian and Carian do not clearly go back to PA, to PIH, or to Pre-PIH, it is nevertheless worth saying that, apart from Greek and Phrygian, *all* the languages attested in southern and western Anatolia, whether in the Bronze Age or the Iron Age, seem to have come from a single tradition. Stated another way, a consensus is now forming that although before the arrival of Phrygian and Greek none of the languages spoken in southern and western Anatolia belonged to the Indo-European family, all were descended either from PA or from a language ancestral to PA. In southern Anatolia even such barely discernible languages as Pisidian and Sidetic, attested by only a few dozen inscriptions on tombs, coins, or dedications, seem to have Anatolian connections in so far as they can be analyzed at all.[28] Sidetic is the name given to a language last heard on the coastal plain of ancient Pamphylia, near the mostly Greek-speaking cities of Side and Aspendus. It is preserved in six inscriptions. In the mountainous interior north of Pamphylia lay Pisidia, a "region of mountains, deeply incised river valleys, and flat, alluvial highland plains."[29] Pisidia wa so inaccessible that even the Persian satraps left it alone. Like the rest of Anatolia, this area too was eventually Hellenized and Christianized, but not before the last generations of Pisidian speakers (having finally been introduced to the alphabet) inscribed a few of their names on tombs. Pisidian is thus preserved in thirty inscriptions, but all of them were found on tombs and "contain only the names of the tomb occupants and patronymics."[30] The personal names seem to be of an "Anatolian" type, as are such place-names as Sagalassos, Ariassos, and Termessos.

Since the 1920s scholars have been aware of the Anatolian affiliation of languages in the main corridor or the "land bridge" leading from the Bosporus and

Dardanelles to Syria. Only now, thanks to specialists in Carian and other minor languages, can we be quite sure that the languages in the nooks and crannies of Anatolia's southern and western coasts came out of the same linguistic evolution as that which led to the Bronze Age languages of the interior. Some of these minor languages may have evolved from PA. Carian and Lydian, however, may have evolved from languages that were cognate to PA or even to PIH and that by 3000 B.C. may have been unintelligible to PA speakers.

LUWIAN AND LYCIAN

Of the three Anatolian languages attested in documents from the middle of the second millennium B.C., the most widespread by far was Luwian.[31] When Hittite scribes wrote in that language, they prefaced what they were about to write with the adverb *luwili*, "[the language] of Luwija." Once literacy began in the second millennium B.C., Luwian is attested (although sporadically) from Troy to Cilicia, and in the early first millennium it was spoken also in northern Syria. Onofrio Carruba drew a diagonal from the Bosporus to the Gulf of Alexandretta and suggested that in the Middle Bronze Age the lands south of this "diagonale anatolica" were Luwian speaking.[32] Lydia and Caria were exceptions, as we have seen, and so was Lycia; and it is of course true that even where Luwian inscriptions are found, we have no guarantee that people in the vicinity spoke Luwian. Nevertheless, it is likely that in the Middle Bronze Age a very considerable part of southern and western Anatolia was Luwian speaking. Over so large an area, not surprisingly, the language took the form of several regional dialects: specialists inform us that Cuneiform Luwian, found at Hattusas, is significantly different from the Hieroglyphic Luwian that shows up especially in inscriptions in Cilicia and, later, northern Syria.

The location and extent of Luwija is not certain. In the Old Kingdom law code, Luwija was one of the three districts, or provinces, over which the Great King at Hattusas ruled. The other two were Pala and Khattuša itself. Pala seems to have lain directly west of the capital, across the Kızılırmak (Halys) river, while *Khattuša* was a synonym for *Hatti*, and stood both for the city of that name and for the land that it dominated: all the territory within the Kızılırmak's arc. Luwija was certainly on the river's left bank and probably extended as far to the south and southwest as did the Great King's authority (see fig. 1).[33] Each of the three provinces had given its name to the language anciently spoken there: Luwian in Luwija, Palaic in Pala, and Hattic in Hatti. In Hittite New Kingdom texts the place-name *Luwija* was no longer used. In the New Kingdom revision of the law code *Luwija* was replaced by the name *Arzawa*. Arzawa, however, seems to have been a much larger province than the Old Kingdom Luwija had been and may at times have extended all the way to the Aegean.

Kuniholm makes the very important point that in the Early Bronze period Anatolia was heavily populated in comparison to the lands to the west (Greece) and to the north (Thrace and the Pontic plain), the *Tübinger Atlas* showing more than fifteen hundred Anatolian sites in which evidence has been found for occupation in the EB (and especially the EB II) period.[34] Most of these are in the Luwian-speaking zone of southern and western Anatolia. My guess is that even in the third millennium several hundred thousand people spoke one or another dialect of Luwian. In southern Anatolia settlements have been found especially in Cilicia, bounded by the sea and the Taurus mountains, and along the western edge of the Konya plain, which begins in the south at the Taurus mountains and continues north until it dries up in the barren basin that surrounds the salt lake called Tuz Gölü. Although Luwian was spoken in Cilicia, so was Hurrian, and in any case the place was even in the Old Kingdom known to Hittite scribes as Kizzuwatna.[35]

Luwija is more likely to have been the Middle Bronze Age name for the arable portions of the plateau that began just to the north of the Taurus mountains and that extended some two hundred and fifty miles to the northwest, to the headwaters of the Porsuk river in the vicinity of present-day Eskişehir.[36] The southeastern part of this large plateau lies south and southeast of Konya and is sometimes called the Konya plain. In the lee of the Taurus mountains, it receives some rainfall, and in the EB it supported dozens of settlements, most of them along a line roughly parallel to the highway that now runs between Konya and Karaman. The northeastern part of the Konya plain, known as Lykaonia in the Iron Age, becomes progressively drier as it nears the Tuz Gölü, and in the Bronze Age it attracted few settlers.[37] Although the plain southeast of Konya was very likely part of the land that Hittite scribes knew as *Luwija*, another and much larger part must have lain to the west and northwest of Konya. That area, most of which King Midas and the other Phrygian kings made into a land that the Greeks called "Great Phrygia," included the roughly triangular "lake district." The southeastern apex of the triangle lay near Konya and the southwestern apex approximately fifty miles southwest of Burdur, in fairly mountainous terrain. Toward the northwest the triangle extended through the Eskişehir plain.[38] In the third millennium B.C. this entire "triangle" was heavily populated, producing evidence for several hundred EB sites. Along with the Konya plain, it probably constituted most of the province that Old Kingdom Hittite scribes called *Luwija*.

Hattic and Palaic seem to have disappeared as spoken languages by the middle of the second millennium B.C., but Luwian was more robust. (Luwian may, in fact, have helped to make Hattic and Palaic obsolete.) Eventually Luwian was replaced in northwestern Anatolia by Phrygian, which by the fifth century B.C. may have been spoken as far to the southeast as Konya (Ikonion),[39] and after Alexander's conquests the Greek language spread rapidly. Nevertheless, the Luwian tradition persisted in Lykaonian, which presumably was its last manifestation.[40] In Acts of the Apostles we read that when Paul and Barnabas worked a miracle in Lystra, the crowd shouted their praises in Lykaonian. Apparently nobody ever wrote anything in Lykaonian, but as late as the sixth century a few rustics in south-central Anatolia still spoke it and knew no Greek.[41] Thus a duration of at least 2500 years for this linguistic tradition seems assured. Looked at another way: the Lykaonian shepherd miraculously cured by St. Martha of Antioch in the sixth century spoke a language that can be traced back in Anatolia as far as written evidence can take us.

Lycian was closely related to Luwian. Margalit Finkelberg notes that Lycian has even been called "West Luwian," although it certainly was—as Finkelberg emphasizes—a distinct language and not a Luwian dialect. In phonology, for example, Finkelberg points out that whereas Luwian lost the phoneme *e*, Lycian kept it (as did Hittite and Palaic). In general, Lycian seems to have been less innovative than Luwian. Carruba found that "in einigen Fällen zeigt das Lykische des 5. Jhs. konservative Züge, die älter als die entsprechended Entwicklungen des Luwischen im 2. Jt. sind und eher auf das Frühanatolische zurückgehen."[42] We may assume that by 2000 B.C. Lycian was already well differentiated from Luwian.

What linguists call Lycian gets its name from the place of its discovery: the land known to the classical Greeks as Λυκία or Λυκίη. At the end of the Bronze Age men from *Lukka* were conspicuous among the aggressors who caused problems for kings in Egypt and the Near East.[43] The evidence for Lycian consists mostly of funerary inscriptions dating from the fifth and fourth centuries B.C. and found in or near the valley of the Koca (Xanthos) river, which springs from the mountains and flows due south for forty miles until it reaches the Mediterranean (see map 2). In contrast to the Konya plain and the Konya-Burdur-Eskişehir triangle, classical Lycia was a linguistic pocket, a mountainous protuberance into the Mediterranean. Lycia has produced even less material evidence for habitation in the early periods than has Caria. Although documentary and literary references show that Lycia must have been inhabited in the Late Bronze Age, whatever settlements were there in the Neolithic period, the Bronze Age and even the early Iron Age have not yet been found.[44] Its archaic character, nevertheless, implies that the language spoken in the Koca valley must have split off either from PA or from Proto-Luwian at an early date. Remote as it was, the Koca valley must have been a part, although perhaps a very small part, of the important land that Hittite scribes knew as *Lukka* (or more precisely, in the syllabic cuneiform, as *lu-uk-ka-a*).[45]

In the second millennium B.C. the land of Lukka was distinct from Luwija, but Carruba's conclusion is that at a much earlier time most of southwestern Anatolia was known as **Lukija*, a name that came to be pronounced differently in two linguistic traditions: in the more innovative Luwian language the palatal changed to a labial and became *Luwija*, while in the more conservative Lycian tradition the palatal remained, and became the classical Λυκία.[46] The people who made their way to the valley of the Koca may originally have spoken a Proto-Anatolian identical to that spoken in the interior, but in the cul-de-sac of the Koca their descendants' language must have become increasingly differentiated from what in the mainstream was evolving into Luwian.

Hittite and Palaic

Ivanov demonstrates the degree to which Hittite differed from Luwian, but perhaps both of them can be derived from PA and their differences explained by

Sprachgeographie.[47] Early Hittitologists discovered that the language that they had unfortunately misnamed "Hittite" was in fact called "the language of Kanesh" by the people who spoke it.

> They spoke of their language as *kanišumnili*, "in the language of the Kaneshite," a word attested only once, or more often as *nešumnili*, "in the language of the Neshite," or *našili/nešili/nišili*, "in the language of (the town) Nesha."[48]

Hrozný's excavations at Kültepe in 1925 produced evidence that ancient Kanesh, or Nesha, lay at modern Kültepe. (Although usually called *Nesha* by Hittite scribes, the city had been known as *Kanesh* at the beginning of the second millennium, and that is how I shall refer to it here.) The meaning of the term *the language of Kanesh* was thus entirely clear already in the 1920s. The meaning was ignored, however, because it was incompatible with the assumption that Hittite was the language of a Hittite nation, freshly arrived from the Pontic steppe. That "Hittite" could have been the language of an urban population in Cappadocia was in fact so contrary to the presuppositions of nationalist scholars that the possibility was hardly considered at all. Today, however, the fact that Hittite was "the language of Kanesh" should be the starting point for attempts to determine where the Hittite language came from.

Kültepe (ash mound) lies some ten miles south of the Kızılırmak (Halys) river and twelve miles northeast of Kayseri (ancient Caesarea).[49] Across the river and two days away was Alishar, perhaps the main center in southern Hatti. In Old Assyrian times Kanesh was evidently the center of a vast exchange network. Approximately twenty thousand tablets, written in Old Assyrian, have been found in the Kanesh *kārum*, a trading quarter that may have extended over eighty acres. On the mound west of the *kārum* lay the upper city, in which the king's three-acre palace took pride of place. Kanesh was thus a large city, and at the beginning of the second millennium it may have been the very largest city in all of Anatolia (there being no other archaeological site in Anatolia, for this period, comparable to Kültepe). This distinction would have been recent, however, since the earlier levels (IV and III) at Kültepe have not produced anything like the *kārum* found in Levels II and I. Although Kanesh may have been a modest city until a century or two before 2000 B.C., it evidently had been occupied all through the third millennium. According to Klaas Veenhof, "The earliest occupation on the mound goes back to the Early Bronze Age and seems to cover (according to a deep sounding) all phases of that period."[50]

In the third millennium Hattic was presumably the language of much of Cappadocia, as it was of all of Hatti. Enough Hattic names were found in the Kanesh tablets to lead some scholars to the mistaken conclusion that before "the Hittite invasion" Hattic was the city's main language (until recently it was supposed that the Anittas text had originally been composed in Hattic and was later translated into Hittite). Another piece of evidence for Hattic speech on the left bank of the Kızılırmak is the fact that Palaic, like Hittite but independently of it, borrowed many Hattic words (whereas Luwian, in contrast, borrowed very few).[51] Still another pointer to the Hattic presence south of the Kızılırmak is the ubiquity of Alishar III ware ("Cappadocian Ware"), suggesting that for material culture the river was not much of a barrier.[52]

Although Kanesh may have been surrounded by villages in which the inhabitants' first language was Hattic, the main language in the city itself was Hittite. That should be absolutely no surprise, since for people who spoke it the name of their language was "the language of Kanesh." If verification is required, it can be found in the Old Assyrian tablets removed from the *kārum*. The personal names mentioned in the tablets indicate that Hittite was the first language of most of the people who lived at Kanesh ca. 2000 B.C.[53]

We may therefore suppose that in Kanesh, Proto-Anatolian had evolved in a rather different way from the way that it evolved in Luwija. In other words, the language we call Hittite seems to have developed in Kanesh itself, and I would suggest that its separate development was the result of the linguistic isolation of the Proto-Anatolian speakers who established the city. Kanesh lay considerably to the northeast of Carruba's "diagonale anatolica," and in some periods communication between Kanesh and the Luwian mainstream in southern and western Anatolia may have been very difficult. It has been noted that Hittite was in some respects more conservative than Luwian.[54] Whereas Luwian had simplified its nominal inflection to five cases, Hittite preserved eight or nine. Melchert and Lehrman estimate that PA was spoken ca. 3000 B.C., and in that same spirit we might imagine that it was at about that time, or even in the somewhat earlier Uruk period, that a PA-speaking population established itself at Kültepe and initiated the separate linguistic evolution that by 2000 B.C. had resulted in Hittite.

The later extension of the Hittite language into the Kızılırmak arc, where it contributed to the extinction of Hattic as a spoken language by the middle of the second millennium, was undoubtedly facilitated when a series of Hittite-speaking Great Kings made themselves politically supreme in Hatti. Anittas in the eighteenth century B.C., who may have had his palace in Kanesh for a part of his reign, destroyed the fortress of Hattusas, and a century later Labarnas (Hattusilis I) rebuilt the site and made it the capital of his Great Kingdom of Hatti.[55] However common "the language of Kanesh" may have become in Hatti (and it is not certain that it ever was a *lingua franca* there), its success can be explained easily enough by the prestige that it would have enjoyed as the Great King's language. There is no evidence that Hatti was ever overrun by a migration or invasion of Hittite speakers, and it is in any case now quite clear that "the Hittite nation" was from the beginning a scholarly mirage.[56]

Palaic was for a while, at least, the language of the province called *Palā* in Old Kingdom texts, a place evidently to be identified with some or all of the land known as Paphlagonia in Hellenistic and Roman times. Extinct as a spoken language by the fourteenth century B.C. and possibly even by the sixteenth, Palaic was preserved as a cultic language for the delectation of the god Zaparfa.[57] The evolution of Palaic, as well as Hittite, from PA can be explained by geography. Paphlagonia's eastern and western borders were the Sakarya (Sangarius) and the lower course of the Kızılırmak rivers respectively. On the south Paphlagonia had no water frontier, effectively insulated by the dry wasteland that bottoms out in the Tuz Gölü. Proto-Anatolian speakers who drifted to the north of the barren Tuz Gölü basin would have been effectively cut off from the populous, Luwian-speaking lands to the south and west, and we can imagine that in that isolation the language they brought with them evolved into the Palaic known from the Hattusas tablets.[58] One of the influences that differentiated Palaic from Luwian came from Hattic speakers. By the Hittite New Kingdom, Palaic had probably given way to a dialect of Luwian.

Because Hittite and Palaic seem to have been colonial languages, differentiated from Luwian because they had "hived off" from the mainstream in which Luwian was to evolve, we may suggest more precisely that the PA speakers who launched the evolution of the Hittite and Palaic languages had come to Kanesh and to Pala from Luwija, which is to say that they came from the Anatolian plateau that stretched between the Taurus mountains and the headwaters of the Porsuk river. Arguments from *Sprachgeographie,* I think, suggest that it was on this plateau that PA had evolved. In addition, those arguments seem to support the notion that while PA was evolving in the interior of Anatolia, languages on the Aegean coast cognate to PA were on their way to becoming Lydian and Carian.

Dating PIH

Our colloquium included a productive discussion of the date at which PIH was spoken. In his presentation Darden looked closely at a few cultural items that—because there were words for them in the PIH lexicon—were obviously known to the PIH speakers. The introduction of these cultural items into human society is, at least in theory, archaeologically dateable, and so the words could give us a *terminus post quem* for PIH. The yoke, the domesticated horse, and wool appeared during what Andrew Sherratt has called the "secondary products revolution," a long period that may have included most of the fifth and fourth millennia B.C. Because the PIH speakers had words for *wool, yoke,* and *horse,* Darden concludes that we can place "a *terminus post quem* for the division of PIH in the late fifth millennium B.C." Similarly, because of the absence in PIH speech of a word or words indicating a wheeled vehicle, a wheel, a nave, or an axle, all of which items were of course familiar to PIE speakers, Darden proposes that PIH could not have been spoken later than ca. 3500 B.C.

The horse is problematic as a cultural item, since we cannot be certain whether the horses about which PIH speakers spoke were domesticated or wild. There was general agreement, however, with Darden's argument that because the PIH speakers had words for *wool* and for *yoke* we may conclude that they raised woolly sheep and employed ox traction. That argument is potentially a major contribution. So far as wool is concerned, Darden agrees with Barber that the PIH speakers did not weave their wool. Instead, he proposes that they used wool for felting. Darden and Lehrman agree that **HwlHn*, the PIH word for *wool*, was etymologically related to words meaning ***to turn*** or ***to twist*** and began as a word for, as Lehrman put it, "the spun stuff." Renfrew proposes that the PIH word may have denoted "the hair of the sheep" rather than proper wool, a suggestion allowing that PIH speakers were speaking about **HwlHn* long before they began to breed woolly sheep. Even the coarse hair of an early Neolithic sheep, however, if it was of some value as a "secondary product," would likely have saved the sheep from being slaughtered immediately on reaching adulthood. Whether **HwlHn* meant "wool" or "hair of a sheep," the crucial question is, When did people begin to raise sheep for their secondary product, rather than for their meat?

That question is not precisely answered by archaeological evidence. Sheep may have been bred for their wool or hair in southern Iran as early as 7000 B.C. (Darden, p. 196; accepted by Barber, p. 2). Barber proposes that in Anatolia and points north woolly sheep are not attested until ca. 4000 B.C., but Darden suggests that they may already have been raised on the steppe north of the Black Sea in the fifth millennium. Although Darden finds no evidence for woolly sheep in Anatolia at so early a date, he notes V. Shnirelman's argument that woolly sheep were introduced to the steppe through the Caucasus. In this connection it is pertinent to note that recent excavations at the site of Domuztepe, in southeastern Turkey, have produced evidence suggesting that the inhabitants of this site may have been raising woolly (or hairy) sheep in Halafian times, with calibrated dates early in the fifth millennium B.C.[59]

The earliest certain evidence for the yoke comes from the middle of the fourth millennium, but again Darden concludes that ox traction, and so the yoke, probably goes back to the fifth millennium and notes (p. 192) that K. Kushnareva has argued for its existence already in the sixth. Renfrew makes the point that our evidence for yokes is tied quite closely to our evidence for wheeled vehicles and suggests that the plow may have been around for a long time before wheeled vehicles. There is much to be said for that suggestion, since the plow (like the sledge) was a relatively simple machine, upon which the wheeled vehicle was a complex elaboration. Osteological evidence for castrated bulls has been claimed for various sites in the fifth millennium B.C., and so it may be safest to assume that already during the fifth millennium yoked oxen were being used for traction of plows or sledges.

Nevertheless, even after allowances are made, and even though a future excavation may push back the date for the yoke or for woolly sheep by several centuries, Darden's arguments make it difficult to believe that PIH could have been spoken before 5000 B.C. If that is correct, present evidence suggests that PIH would still have been undivided six hundred years after the Black Sea Flood, which William Ryan and Walter Pitman have dated to ca. 5560 B.C. Unless the date of that flood is radically pushed forward by new carbon dates,[60] or unless the dates for yokes and for sheep that were valued for their "spun stuff" are pushed back, speculation that the Black Sea Flood may have separated PIE from PA will probably have rough going.

In contrast, a date for PIH close to Darden's *terminus ante quem* of 3500 B.C. is also, I think, not very likely. That is uncomfortably close to the time at which PIE is widely supposed to have been spoken (Mallory's arguments would support a date ca. 3000 B.C. for what in this essay is called PIE).[61] As we have seen, Lehrman and Melchert suggest ca. 3000 B.C. also as a date for PA and for the beginning of its divergence into branches that would culminate in Luwian and Hittite. If we look at Ivanov's tables, we can see what the PA lexicon would have looked like for a fair number of basic items. For some of these—fire, foot, water—the PA word is reassuringly similar to the PIE word and, indeed, to our own English word. But for most of the items in the basic vocabulary list, that is not the case, and either the PA branch or the PIE branch or both must have done a great deal of innovating between the binary split of PIH and the maturation of PIE and PA. So much linguistic change in five hundred years would be surprising.

Arguments for the high antiquity of PIH are also supplied by Lehrman's paper. Lehrman gives us his catalogue of the innovations in phonology and morphology that occurred in the PIE branch (pp. 114–16) and in the PA branch (p. 117) between the time that these branches split from each other and the time that they culminated in PIE and PA. Although other specialists will have their own catalogues, readers who accept Lehrman's list (in PIE, the loss of PIH laryngeals, the addition of a feminine gender, the development of active and passive participles, and much else) must guess for themselves how much time must have elapsed between PIH and the maturation of its PIE daughter. (Lehrman's own guess is about two thousand years.) Equally informative is Lehrman's reconstruction of the PIH lexicon. Here we find a word for *grain*, but not for *wheat*, a word for *tree*, but not for the specific kinds of trees (birch, beech) known from PIE. Bees, bears and bovines are attested in the PIH lexicon, but not the wide array of animals attested for PIE: ducks, geese, pigs, goats, lambs, bulls, deer, and hares, as well as (generic) fish and (specific) salmon. Nor is PIH known to have been the source for many fairly basic cultural or technological words that are attested for PIE: Lehrman could reconstruct no PIH word for *wheel*, *metal*, *boat*, or *bridge* or for activities such as *to sow*, *to cook*, *to weave*, or *to build*. Numerals from one to ten should be imperturbably stable; so what is one to make of the fact that our word *four* goes back all the way to the PIE speakers' _*k^{w}etwóres_. while the PA speakers' word for *four* was _*maw-_? Obviously, either in PA or in PIE the word that had been inherited from PIH for this fundamental numeral was replaced with something very different.

Most surprising of all, in Lehrman's survey, is the narrow range of kinship terms that PIE and PA shared. From the PIH lexicon came the PIE words for *daughter* and *grandfather*, but the PIE kinship terms that cannot be reconstructed for PIH include the words for such familiar persons as *father*, *mother*, *brother* and *sister*. Such discrepancies suggest that between the time that PIH was spoken and the time that PA and PIE were spoken, a very long time had elapsed. At one point (p. 120) Lehrman suggests that PIH was spoken ca. 5000 B.C. That may be difficult to sustain against the present lack of material evidence for yokes and for woolly sheep at so early a date. But the lexical argument (together with the argument from morphology and sound changes) does suggest that the date for PIH is likely to have been considerably closer to 5000 than to 3500 B.C. Perhaps we can say tentatively that PIH (defined, of course, as the language being spoken *immediately before* it diverged into two trajectories, leading respectively to PIE and PA) seems to have been spoken at some time in the fifth millennium B.C.

Ramifications of the dating of PIH

Although even an early fifth-millennium date for PIH would be too late for us to suppose that the divergence of PIE and PA could have been a consequence of the Black Sea Flood, it may be premature to rule out that possibility completely. Carbon dates and tree-ring dates will soon be obtained from coastal settlements drowned in the flood, and although they may not approach the 2700 B.C. date suggested by Kuniholm on the evidence from Kiten, they may push forward significantly the carbon dates (5600 B.C., calibrated, and derived mostly from molluscs) available to Ryan and Pitman. In the other direction, earlier evidence for woolly sheep and for yokes would push back the *terminus post quem* for PIH. The flood was evidently a major cataclysm, covering thousands of square miles of what had been the coastlands around the "Black Sea Lake," and so is likely to have driven tens of thousands of people to new places to settle. One can imagine various scenarios in which such a large-scale displacement could have led to the schism within PIH that ultimately resulted in PA and PIE.

Quite apart from any remotely possible connection with the Black Sea Flood, a fifth-millennium date for PIH has interesting ramifications, but we must be careful in spelling them out. According to Darden (p. 204), "One thing we can say is that this date eliminates the proposals by Renfrew and Drews (1997). At the time of the migration of farmers from Northwest Anatolia to Greece, there was no ox traction, no wool technology, and no trace of horses on either side of the Bosporus." That last objection, as is shown in the appendix to this volume provided by Geoffrey Summers, is not quite correct, since horses were apparently known in early Neolithic Anatolia. But Darden is undoubtedly right to assert that when the first farmers moved from Anatolia to Crete and Greece, during the seventh millennium B.C. (or possibly even at the end of the eighth), ox traction and the working of wool were not yet known.

Nevertheless, the conclusion that Darden draws from that fact seems invalid. Although the PIH lexicon included words for *wool* and *yoke*, it is unlikely that either of those words was used by the speakers of Pre-PIH, the early Neolithic language from which PIH evolved. And in any discussion about the eighth or seventh millennium B.C., the language we must be talking about is not PIH—as Darden himself has now so clearly shown—but Pre-PIH. In 1987 Renfrew argued (1) that the language spoken in the Konya plain at the beginning of the Neolithic period spread with agriculture to southeastern Europe and (2) that as this language, which Renfrew identified as PIE, proliferated in Europe, a series of "transformations" produced—beginning with Greek—the several branches of the Indo-European language family. This part (2) of his thesis has been roughly handled by linguists, and although Renfrew in this volume revises it considerably, it is still likely to be unacceptable for most linguists and even for many archaeologists. One obvious objection—that nobody, whether on the Konya plain or anywhere else, could have been speaking PIE so early as 7500 B.C.—is largely a response to Renfrew's elastic definition of "PIE." A more damaging objection to (2) is that any diachronic series of "transformations" is difficult to square with the apparent fact that *all* of the branches of the Indo-European language family inherited a common set of innovations. In other words, before fissuring into subgroups, PIE speakers seem to have remained in a single language community for a very long time after the division—no earlier than ca. 5000 B.C.—that led to PIE and PA. To get round this, Renfrew proposes that the innovations occurred within a *Sprachbund* located in "Old Europe," but another difficulty is that southeastern Europe and western Anatolia seem to have shared a good many elements of material culture in the Neolithic period. One would therefore not expect that the inhabitants of southeastern Europe (extending, if we accept the arguments of Barber and Darden, as far east as the Dnieper) were so isolated from PA in the Neolithic period that their language(s) could have become so markedly differentiated from the languages of Anatolia as PIE was from PA.

The first part of Renfrew's original thesis has fared much better than the second. Although few archaeologists have agreed that a culture originating in the Konya plain eventually and over the millennia spread to western Europe, there is considerable support for the thesis that such a culture did expand through the Balkans and possibly as far as central Europe. This more limited argument has been made by Andrew and Susan Sherratt (1988), by Broodbank and Strasser (1991), by Marek and Kamil Zvelebil (1990), again by Marek Zvelebil (1995), and is broadly accepted by Renfrew himself in this volume. In particular, the material evidence of fauna and flora and of artifact typology strongly suggests that a population from southern or western Anatolia did come to Crete, to the Greek mainland, and to the southern Balkans at the beginning of the Neolithic period. The first part of the Renfrew thesis—that the language of western Anatolia would have come to the Aegean and southeastern Europe along with this early Neolithic population—was not only still standing at the conclusion of our colloquium but had even been modestly strengthened.

Onomastics have long suggested that early in prehistory a non-Greek language spread from western Anatolia into the Aegean, leaving as mementos place-names ending in *-ssa* and *-nda* (or, in their later and Hellenized forms, *-ssos* and *-nthos*). The general consensus among archaeologists that a population movement from Anatolia through the Aegean occurred at the beginning of the Neolithic period raises the likelihood that it was at this time that the *-ssa* and *-nda* names were brought to Crete, Greece, and other areas of southeastern Europe. If so, the language in question could not have been PIH (to say nothing of PIE) because PIH did not yet exist in the eighth or seventh millennium B.C. We can be quite certain that one of the languages spoken at that time was Pre-PIH, but *where* Pre-PIH was spoken is, of course, not certain at all. As Rutter has noted (p. 244), Crete is crucial to the problem and may hold the key to its solution. Rutter himself believes that despite obvious disasters (such as the destruction at the end of EM II, or the eruption of Thera) the material evidence suggests a continuous evolution of society on Crete between the island's original colonization, no later than 6500 B.C., and the culmination of the "palaces" in LM I, five thousand years later. The implication, Rutter reminds us, is that the language of the Linear A tablets would have evolved from the language brought in by the first settlers. What language did these first settlers speak?

In her analysis of the probable linguistic affinities of the language of the Linear A tablets, Margalit Finkelberg shows the difficulties in relating it either to the Indo-European or to the Semitic family, and she concludes that this "Aegean" language had more parallels with Lycian than with any other candidate thus far proposed. Melchert, in turn, points out several difficulties in Finkelberg's argument, and at present we may have to place a *non liquet* on the possibility that the language of Crete had ties specifically to Lycian. Most of Finkelberg's arguments, however, apply not to Lycian specifically but to the Anatolian language family in general. And some of her arguments (on phonology, for example) seem to suggest that the Cretan language shared more with the most archaic Anatolian languages than it did with the innovating Luwian. Ties between Cretan and the languages of southwestern Anatolia in general may be a good bet.[62]

It is possible, for example, that the roots of the language of Crete (and of much else in the Aegean) may go back to Caria. Ancient writers, as is well known, linked the Cretans even more closely with the Carians than with the Lycians, and it seems that in the EB period—the earliest period for which we have any evidence—Caria maintained close connections with the islands of the Dodecanese.[63] The word *labyrinthos*, which supposedly meant the "place of the double axe," has an almost exact parallel with *Labraunda*, the name of the Carian cult-site in which—as numismatic evidence shows—a double axe was also important.[64] As Pierre MacKay remarked in our Sunday morning discussion, none of the speakers made much reference to onomastics, and onomastics—the undeniable fact that place-names in Crete, the Greek mainland, and beyond share the same suffixes found in place-names throughout western and southern Anatolia, and the likelihood that the suffixes belonged to a language ancestral to the Anatolian group[65]—add a very strong argument that the pre-Greek language of the Aegean was ultimately related to the languages spoken in western Anatolia. The recent decipherment of Carian allows us, finally, to say that until the arrival of Turkish *every one* of the languages that are known to have been spoken in western Anatolia—whether in the Bronze Age or in the Iron Age—belonged to the family of languages descended from Pre-PIH. That the language spoken in Crete (and, presumably, elsewhere in the pre-Greek Aegean) was also descended from Pre-PIH is therefore very likely.

Should the language of the Linear A tablets be convincingly tied to the languages of southwestern Anatolia, it would be difficult to avoid the conclusion that Pre-PIH was the language of western Anatolia when the Neolithic period began. In the present state of our ignorance this cannot be more than a theory. But it is possible and even, I think, probable and certainly must be kept in mind as study continues. The theory would be that a population expanding westward from the Anatolian plateau brought with it a language that in southeastern Europe diverged into several (and possibly into quite a few) languages, all of them having descended from Pre-PIH. None of them, however, was or would become an Indo-European language, for the simple reason that all of them would have been directly descended from Pre-PIH, not from PIE. The "Minoan" language of the Aegean may have been one of these descendants from Pre-PIH. Another, as Finkelberg has observed, may have been Etruscan.[66] Etruscan is clearly not an Indo-European language, nor is it likely to have belonged to the Indo-Hittite family. At this stage of the discussion, however, it would be unwise to rule out the possibility that the roots of Etruscan may go back well beyond PIH, to the Pre-PIH language of the eighth or seventh millennium B.C.

Placing PIE

In contrast to other languages descended from PIH, however, PIE is not likely to have evolved from a language carried westward from Anatolia in the eighth or seventh millennium B.C., or even the sixth. If PIH was not spoken before 5000 B.C., then the splitting of PIH into two traditions (which after millennia would culminate in PA and PIE respectively) must have resulted from a process or event that occurred after 5000 B.C. I have argued above that the evolution from PIH to PA, an evolution notionally dated ca. 5000–3000 B.C., occurred on the Anatolian plateau, and more specifically in the land that during the Middle Bronze Age was known as *Luwija*. If that argument is correct, we would have to assume that at the same time, between ca. 5000 and 3000 B.C., PIH evolved into PIE at some place other than the Anatolian plateau. Where that may have happened remains a wide open question.

Our colloquium aired three competing arguments about where PIE originated. Renfrew proposes that it was in Old Europe (although not quite the same as Gimbutas's Old Europe; see Renfrew's map at his figure 2, p. 43) that "narrow PIE" evolved from the language brought to Europe from Anatolia at the beginning of the Neolithic period. Renfrew's "narrow PIE," it will be recalled, is not a language but a *Sprachbund* of cognate languages: these several languages together adopted the innovations that set them apart from the Anatolian languages. Darden and Barber, adhering to the more conventional definition of PIE as a single language, argue that it evolved north of the Black Sea and necessarily east of the Dnieper. Barber stresses the lack of terminology in PIE for large looms, observes the considerable evidence for large, warp-weighted looms west of the Dnieper in Neolithic times, and concludes that PIE could not have evolved west of the Dnieper.[67] Very interesting is Barber's argument that the plaid twills found at Ürümchi and at Hallstatt have such striking parallels that they must be presumed to have come from the same weaving tradition. She suggests that the men *and women* who brought the seeds of the Tocharian language to Asia and of the Celtic language to Europe had roots in "Caucasia," where early third-millennium B.C. prototypes of the plaid twill have been found.

If Barber and Darden are right that PIE was spoken east of the Dnieper, how it got there and how it separated from PIH would still be open questions. On those questions Darden's and Barber's reconstructions contrast sharply with Renfrew's. In Renfrew's scenario the *Sprachbund* that he calls "narrow PIE" evolves from a language that a demic expansion brought out of Anatolia and into southeastern Europe at the beginning of the Neolithic period, and PA evolves among the people who stayed at home. Opposing that view, Darden and Barber have both PA and PIE evolving on the Pontic steppe in the Neolithic period, and they bring the several Anatolian languages into Anatolia by means of migrations. Barber brings the Luwians to Anatolia (via Troy) and to Crete ca. 3000 B.C.; she brings Proto-Greek to Greece via the southern Balkans ca. 2300 B.C., and the Hittites to central Anatolia well after the Luwians were in place. Darden (p. 220) reverses the sequence. He proposes, on the one hand, that in the fourth millennium B.C. people moved from the southern Balkans into northwest Anatolia and on—past Demirçi Hüyük—into central Anatolia, where they would eventually speak Hittite; he supposes that the Luwians, on the other hand, made their destructive way into Anatolia in the middle of the third millennium B.C. For reasons already given, I think it more likely that Hittite and Luwian evolved from PA in Anatolia itself, over the course of the third millennium B.C. As for migrations, while many Anatolian sites were no doubt destroyed at the end of the EB II period, very little archaeological evidence suggests that this was the result of migrations from either the southern Balkans or the Pontic steppe.[68] Darden and Barber may be right that PIE was spoken on the Pontic steppe east of the Dnieper. If so, however, the explanation may be that PIH was brought to the steppe by a generally peaceful diffusion from Anatolia. Although Renfrew will probably not persuade most specialists that Old Europe was the place in which PIE evolved from PIH, his understanding of the process by which the evolution occurred may well be correct.

Ivanov still holds that it was in the eastern part of Greater Anatolia that what he calls "PIE" evolved. Although he spent little time advancing or defending that thesis, he assumed it and mentioned Gordon Whittaker's argument that Sumerian seems to have borrowed polysyllabic words from an Indo-European (not merely an Indo-Hittite, but an Indo-European) language.[69] Ivanov's argument for a Kura-Araxes homeland for his "PIE" may derive some indirect support from Paul Zimansky's illuminating presentation on the Great Kingdom of Urartu, centered at Van. Zimansky stresses the artificiality of the kingdom and all its institutions: the entire royal assemblage seems to have been imposed on a region that had no such traditions and that evidently had a quite small population. Although Zimansky suggests that the Armenian language may have been brought to the Araxes valley between 800 and 650 B.C., in the persons of central Anatolian captives who spoke that language, his observations would also (and, I think, even more easily) support the hypothesis that the language imported at that time to the Araxes valley was the so-called "Urartian" language. The royal inscriptions, Zimansky emphasizes, are so formulaic, so limited in expression, and so closely modeled on Assyrian prototypes that they reveal little about the language in which they are written. May the reason for such clumsiness be that "Urartian" was not the native language of the kings and scribes who produced the inscriptions? May "Urartian" have been one more royal institution, imported (as was Assyrian) from the south by Sarduri I and his successors as a medium through which to advertise their accomplishments? Because it is not self-evident that the inscriptions of these kings reflect the "native language" of the land they ruled,[70] it is theoretically possible that the roots of Armenian in this region go back far beyond the Great Kingdom centered at Van.

Although Ivanov's proposal that PIE evolved in the easternmost parts of Greater Anatolia remains a possibility, and although PIE could well have spread from south of the Caucasus to the Pontic-Caspian steppe, that PIH could have evolved in easternmost Anatolia seems less likely. Once again, the "Cretan connection" is crucial. Should Finkelberg and other scholars working with onomastics and the Linear A tablets make a persuasive case that the language of those tablets is indeed related, however remotely, to the Anatolian language family,[71] the most economical explanation for that relationship would be—as Rutter has observed—that it resulted from the colonization of Crete (and many other places in the Aegean) by western Anatolians in the eighth or seventh millennium B.C. A persuasive case that Pre-PIH was spoken in western Anatolia in the early Neolithic period would make it difficult to avoid the conclusion that PIH evolved in the same place (or at least west of the Hattic speaking zone), completing its evolution at some point in the fifth millennium B.C.

Notes

1. Lehrman 1996; Warnow, Ringe, and Taylor 1996.
2. Renfrew 2000, 7 ff.
3. Alkim 1968, 145. This volume appeared in the *Archaeologia Mundi* series.
4. As Hittitologists have explored the corpus of Hittite texts, and as translation has made some of the corpus accessible for non-specialists, belief in a Hittite nation has shriveled to the vanishing point (see below, note 56). In retrospect, it appears that Europeans and Americans

in the 1920s simply assumed the existence of a Hittite nation and saw no reason to question their assumption. The discovery of the Hittite language, and of its "Indo-European" character, was sufficient to place "the Hittites" at the head of the list of Aryan nations.

5. According to Darden, both Luwians and Hittites came from the northwest, the Hittites in the fourth millennium and the Luwians in the third. Barber has the Luwians arriving first, from the northwest, and brings the Hittites in at a later time and from the northeast (through the Caucasus).

6. The presence of Mysian speakers on both sides of the Bosporus and the Sea of Marmara fueled Greek speculation about Mysian migrations, either from the Balkans into Asia or vice versa. The speculation was collected by W. How and J. Wells, *A Commentary on Herodotus* (Oxford: 1912), 2:133–34. Greek writers tended to place such migrations at about the time of the Trojan War. Strabo 12.8.1–3 says that the Mysian language was reputed to have been a mixture of Lydian and Phrygian.

7. On the Bronze Age (including EB) settlements on Chios and Samos see Isler 1973 and Hood 1981–82 and 1990. Dickinson 1994, 308, notes that Chios was not "Mycenaean" in the Late Bronze Age: the first Greek speakers seem to have set themselves up in Emporio after the Bronze Age ended.

8. Greenewalt 1995, 1175.

9. For a complete picture of the evidence for Early Bronze settlements in Anatolia see Map B II 13 (Kleinasien: Frühbronzezeit) of the *Tübinger Atlas des Vorderen Orients*. In the loose sheets of the boxed set this double map (Ostteil, Westteil) has the ISBN number 3-88226-803-4.

10. Mellink 1993, 120.

11. On the recent excavations at Liman Tepe, Panaztepe, Miletos, and Bakla Tepe see Gates 1997, 265–68.

12. The "burnt palace" that stood at Beycesultan for generations in the Middle Bronze period belonged to level V. Below that level were thirty-five strata, the lowest of which "may perhaps be put at *c.* 4600 or 4500 BC (calibrated)," according to Mellaart 1998, 61.

13. Burkert 1985, 149, notes Artemis's "close connections with Asia Minor. Her name appears among the gods of the Lydians and Lycians, and though a borrowing from the Greek is probable in the case of the Lycians and not impossible in the case of the Lydians, the theophoric personal names with their non-Greek formation indicate how fully this goddess is assimilated." In the Artemis cults at Perge and Ephesos, according to Burkert, "Asia Minor elements seem to have been taken over wholesale by Greek cities, in the cult image and also in the organization of beggar priests or even eunuch priests within the framework of a temple state."

14. Greenewalt 1995, 1179.

15. Masson 1991, 670–71.

16. Ibid., 676.

17. Ray 1981.

18. Melchert 1998, 33.

19. In Ray's words, "It emerges that the Carian language is Indo-European, and specifically part of the Hittite-Luwian family. This is what would be expected, given the Carians' own views about their origins and taking into account the evidence of Carian proper names preserved in Greek inscriptions from the homeland and elsewhere. The language seems at the moment to be closer to Lycian than to Lydian, but this may be an illusion; it is likely that all three languages are essentially related, and are descended from western Anatolian dialects of the second millennium" (Ray 1995, 1193).

20. Melchert 1995, 2158.

21. For the quotation see Carruba 1998, 54, and for a change of mind see his post scriptum (55–56).

22. Van den Hout 1999, 31. For claims of similarities to Luwian see Hajnal 1998, 81: "Die karische Sprache ist aus genetischer Warte Mitglied des anatolischen Sprachzweigs, als dessen Hauptvertreter das Hethitische gilt. Innerhalb des Anatolischen erweist sich das Karische jedoch als Vertreter der luwischen Untergruppe."

23. Melas 1988.

24. Ibid., 109.

25. Broodbank and Strasser, 1991.

26. See Map B II 13 (Kleinasien: Frühbronzezeit).

27. For reservations about Lydian see Melchert, p. 231 in this volume: "Despite a number of published statements on my part to the contrary, I would not absolutely preclude that Lydian, which is in many ways very different from the rest of the Anatolian group, reflects a separate tradition whose shared resemblances to the others are due to late convergence."

28. Melchert 1995, 2157.

29. Mitchell 1998, 237

30. Melchert 1995, 2157.

31. On the corpus of Luwian texts see Houwink ten Cate 1995, 267–68. And on the features of Luwian see Melchert 1995, 2154–56.

32. Carruba 1995, 18; cf. Renfrew, this volume, fig. 5, p. 52.

33. On the three provinces in the Old Kingdom see Houwink ten Cate 1995, 268. For details and bibliography on Luwija see del Monte and Tischler 1978, s.v. Luwija (252–53).

34. See Kuniholm (this volume), p. 34 for reference. See also map B II 13 (Kleinasien: Frühbronzezeit) in the *TAVO* set. Compare with map B II 14 (Kleinasien: Mittelbronzezeit).

35. Del Monte and Tischler 1978, s.v. Kizuwatna (211–15).

36. Northwest of the drainage basin of the Porsuk river were mountains, and beyond the mountains and all the way to the Troad and the island of Lesbos the language spoken in the Bronze Age was probably ancestral to the Mysian spoken in Herodotus's day. Whether Iron Age Mysian was descended from Luwian or from a language cognate to Luwian is unknown, since no Mysian inscriptions have been found.

37. Xenophon *Anabasis* 1.2.19 reports that after leaving Ikonion (modern Konya), Cyrus and the 10,000 marched for over a hundred miles through Lykaonia and then reached the mountains beyond which lay Cilicia. For Lystra and Derbe as Lykaonian cities see Acts of the Apostles 14:8–13. For the connection of both *Lykaonia* and *Luwija* to an original **Lukija* see Carruba 1996, 29: "Es muss hier nochmals hervorgehoben werden, dass die sprachliche Grundlage des Names *Lykaonia* nur *Lukka* sein kann, ebenso wie für *Lykia* (und *Lykioi*) eine schon alte, aus *Lukka* abgeleitete **Lukija* anzunehmen ist."

38. For this area see especially Efe 1994. At p. 21 Efe concludes that "nearly all the finds, and in particular the pottery, from sites throughout western Anatolia demonstrate an extraordinary cultural unity during the third millennium B.C. that sets this region apart from neighboring areas." For Efe's map of the ten EB settlements thus far found along the upper Porsuk see his fig. 1.

39. Xenophon, *Anabasis* 1.2.19 calls Ikonion "the farthest city of Phrygia," and it can be assumed that in Xenophon's time the designation "Phrygia" was more or less coterminous with the Phrygian language. Konya is approximately the southeastern limit of the 240 Old Phrygian inscriptions.

40. On the identification of Lykaonia with **Lukija* see n. 37 above. Arguments tying

Lykaonian to Luwian were evidently made by G. Neumann, *Untersuchungen zum Weiterleben hethitischen und luwischen Sprachgutes in hellenistischer und römischer Zeit* (Wiesbaden: 1961), but I have not seen them.

41. *Vita S. Marthae* 41.

42. Carruba 1996, 34.

43. Two of the stalwarts among the Trojan allies, Glaukos and Sarpedon, came "from faraway Lycia, and the eddying Xanthos" (*Iliad* 2.877). For participation of men from *Lukka* in the troubles ca. 1200 BC see Drews 1993, 14 and 49–50.

44. Bryce 1995, 1161: "We have little archaeological evidence for settlement in the country before the end of the eighth century BCE."

45. Bryce 1992.

46. Carruba 1996, 28–30.

47. Although I hope that much in Drews 1997 is still worthwhile, the suggestions I made there about the origins of Hittite are not.

48. Houwink ten Cate 1995, 261.

49. My information about the site is drawn mostly from Veenhof 1995.

50. Veenhof 1995, 859–60.

51. On the likely extent of Hattic see Gene Gragg 1995, 2174–75. Gragg reckons with "an important Hattic population" at Kanesh in the early second millennium.

52. On the Hattic context in which the first so-called "Hittite" kings operated, see Collins 1998, 19: "Hattic influence in the religion of the Hittites was so extensive as to pervade every aspect of the cult in this early period of the development of the Hittite state."

53. Cf. Houwink ten Cate 1995, 262: "Studies undertaken by Paul Garelli and Emmanuel Laroche have verified that the majority of analyzable, non-Assyrian names point to the presence of indigenous Hittites as the most important local group within the native population. A compelling number of recognizable indigenous names, which cannot be analyzed, have unmistakable doublets among later Hittite name material."

54. Cf. Melchert 1995, 2155: "Luwian resembles Hittite in many respects, but is generally more innovative in its grammar."

55. Collins 1998 points out that although the Hattic influence on the first "Hittite" kings was very pronounced, one Old Kingdom text—the so-called Anittas Text—long thought to have been translated into Hittite from a Hattic original is now believed to have been an Old Hittite composition.

56. An analysis of the documents found at Hattusas also points to the conclusion that "the Hittites," like "the Luwians," are constructs of nationalist presuppositions. Rummaging through the 26,000 tablets from Hattusas, Harry Hoffner was hard pressed to come up with a single one that showed either "the Luwians" or "the Hittites" acting as a collective or corporate force. See Hoffner 1980, 331: "I am much less sure that I can identify works of Hittite historiography composed in order to appeal to 'national pride.' My first reaction was to include that motivation along with Grayson's (4) and (5) as absent from Hittite historiography. . . . I would judge that the Hittites had nothing remotely approaching the Homeric *Iliad* in its role as the buttress of national Greek pride and unifier of the various Greek sub-groups."

57. Melchert 1995, 2154.

58. For references to Pala and bibliography see Del Monte and Tischler 1978, s.v. Pala (297–98). One of the texts declares that Pala was a land with no proper cities.

59. Campbell 1999, 413, reports that in the excavations from 1995 to 1997 approximately 50 percent of the bones recovered were ovicaprid. "The kill-off pattern in sheep and

goats, based on both frequency of epiphyseal fusion and mandibular tooth eruption data, shows that animals were being maintained to older ages, typically more than three years, perhaps to be exploited for milk, wool, and hair. . . . The maintenance of a large number of adults and the absence of juveniles might indicate that wool was an important secondary product. This possibility is supported by the extensive range of weaving tools, including spindle whorls, bone awls, needles, and spatulas, found at the site." Domuztepe lies approximately fifty miles northeast of the Gulf of Iskenderun.

60. Ryan and Pitman 1998. Kuniholm's evidence from Kiten in Bulgaria indicates that as late as 2700 B.C. the Black Sea was at least ten meters lower than its present surface. But whether in 2700 BC the Black Sea was a hundred meters lower than it is now is another question. If the Kiten settlement was built on piles to elevate it above the water level, we should be able to conclude that in 2700 B.C. the Black Sea was *not more than* ten meters below its present level. The "great flood," in other words, would in that case have occurred long before 2700 B.C.

61. Mallory 1989, 239, 241 and 262–64. Although Mallory's "PIE" here includes the Anatolian languages, he has them separating from the rest of PIE late in the fourth millennium B.C. More recently, Mallory pointed out that linguists who date PIE as late as 2500 B.C. either must find a mechanism by which PIE could after that date have been dispersed from a small area to most of Eurasia, or must assume that ca. 2500 B.C. PIE was spoken throughout an area stretching from central Europe to the Aral Sea. "Can any linguist seriously envisage a single linguistic entity over such a large area?" (Mallory 1996, 6).

62. Cf. Melchert, p. 230: "I personally find it a priori unlikely that a language found on Crete in the eighteenth to fifteenth centuries B.C. would turn out to be an exact match for one of the attested Indo-European languages of Anatolia. I would expect rather to find yet another dialect with its own peculiar combination of features."

63. Melas 1988.

64. On the *double-hache* as the symbol of Zeus Labraundos see Laumonier 1958, 44, 59–60, 62–63 and 66–69. On the connection between Labraunda and the rulers of Caria see Hellström 1996.

65. Laroche 1957 and 1961; Carruba 1983, 82, and 1995, 18. In Carruba's reconstruction, the "Anatolian" language responsible for the place-names was brought from Greece to Anatolia at the end of the Chalcolithic period.

66. On possible Etruscan affinities with the IH family see Finkelberg 1997, 6–7.

67. Whether the PA lexicon included terms for large looms, Barber notes, cannot be determined from the available Luwian and Hittite texts. Barber also points out that at least in western and southeastern Anatolia vertical ground looms seem to have been used in the fourth millennium B.C.

68. Although belief in mass movements into Anatolia remains strong, advocates tend to date these migrations or invasions considerably earlier than did scholars of fifty years ago. The consensus that "the Hittites" and "the Luwians" invaded Anatolia ca. 1900 B.C. was broken by James Mellaart in 1958, in an article that proposed pushing back the date for "the Luwians" by several centuries, to the interface of the EBII/III periods: although "the Hittites" entered Anatolia from the northeast ca. 1900 B.C., destroying everything in their path, "the Luwians" had invaded Anatolia ca. 2500 B.C., and from the west, coming from the Balkans by land and sea and destroying the Troy I culture (Mellaart 1958, 13–15 and 26). Today, I think, nobody would argue for *any* invasion of Anatolia ca. 1900 or 2000 B.C., and Mellaart himself has now pushed his great population movements back to the beginning of the fourth millennium (Mellaart 1998: 62–63).

69. See Whittaker 1998.

70. That Hurrian was the vernacular of the "Urartian" area in the Bronze Age has long been assumed and was most recently argued by Charles Burney (1989). Although it may be that Hurrian was spoken north as well as south of the Taurus-Zagros range, it is known only that in the third and second millennia B.C. Hurrian toponyms were scattered widely over northern Mesopotamia and northeastern Syria. See Salvini 1998.

71. In addition to her arguments in this volume see Finkelberg 1997; Brown 1992–93.

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APPENDIX

QUESTIONS RAISED BY THE IDENTIFICATION OF THE NEOLITHIC, CHALCOLITHIC, AND EARLY BRONZE AGE HORSE BONES IN ANATOLIA

Geoffrey D. Summers

This brief note seeks to raise several questions that have arisen as the result of the identification of equid bones, including those of horses, from excavation of Neolithic, Chalcolithic and early Early Bronze Age sites on the Anatolian Plateau. It does not propose answers or solutions but, rather, is intended to stimulate those with specialist knowledge in zoology and archaeozoology, and those who are engaged in theoretical studies aimed at bringing together both linguistic and archaeological evidence. The review of evidence does not pretend to be exhaustive, although it is intended to go some way toward providing a sufficient basis for further discussion.

Three questions are explicitly addressed to archaeozoologists:

1. What was the distribution of wild horses in Anatolia during the late Neolithic?
2. How long did populations of wild horse survive in Anatolia after the end of the Neolithic period?
3. What is the evidence for the earliest domesticated horse in Anatolia?

One question is addressed to zoologists:

> Is it possible to distinguish between different types of equid on the Çatalhöyük wall paintings?

Two theoretical questions are also raised:

1. If the osteological evidence for the presence of wild horse from Neolithic levels at Çatalhöyük had been made available as a result of James Mellaart's excavations in the 1960s, how would that have influenced the formulation

and development of Colin Renfrew's theories concerning the spread of Indo-European language and culture from the central Anatolian Plateau into eastern Europe?[1]

2. How, if at all, should Renfrew's thesis be reevaluated in the light of the new evidence and a reexamination of earlier reports?

Received Wisdom

The standard view of the distribution of the wild horse, and of equids in general, is clearly set out in an excellent study by Juliet Clutton-Brock, senior scientist at the Natural History Museum in London, published as recently as 1992. On page 24 she states: "In Europe the wild horse retreated to the steppes of central Asia and by 7000 years ago it was probably a very rare animal in the west." Hunting by man and the spread of forest are thought to be major factors in the demise of the horse in Europe. Clutton-Brock's discussion of the earliest domestication of the horse (53–66) makes no mention of Anatolia.

This widely held view, that horses were absent from Anatolia from the end of the Pleistocene until they were introduced as domesticated animals in the Bronze Age, was unquestioningly accepted by many scholars (e.g., Mellart 1981: 137; Uerpmann 1987: 16). Confirmation that horses were absent from western Turkey and Turkish Thrace before the Early Bronze Age comes from a study of the faunal remains from Ilıpınar, Hocaçeşme, Fırıktepe and Toptepe (Buitenhuis 1995) and from sites in the vicinity of Kırklareli (Benecke 1998).

Earliest Domestication of the Horse: A Development

A long-standing controversy over the domestication of the horse on the Eurasian Steppes as early as the second half of the fourth millennium, based on evidence of tooth wear that was taken to indicate the use of bits, has received a severe knock with the redating of skeletal remains of the horses from Dereivka to the first millennium B.C. (Anthony and Brown 2000). Nevertheless, it is still maintained that the preconditions for horse domestication existed only in the steppes where, it is reported, horse bones account for up to 50 percent of the total bone remains. The suggestion that, east of the Urals, horses were bitted and ridden for the hunting of wild horses by 3500–3000 B.C. has interesting implications.[2]

The New Evidence

The resumption of excavations at Çatalhöyük, under the direction of Ian Hodder, has yielded certain osteological evidence of three types of wild equid amongst the faunal remains: two species of wild ass and wild horse. There are no reports of equid skulls from the remains of refuse, and equid skulls were apparently not incorporated into the plastic art adorning walls. It is thus clear from the osteological evidence that equids (including horses) were hunted and eaten, but they did not attain the symbolic significance attached to cattle (and some other wild animals). It can be fairly assumed, despite the current absence of positive evidence, that the hides and hair would also have been utilized. Louise Martin and Nerissa Russell have generously made the preliminary evidence available on the World Wide Web at

<http://catal.arch.cam.ac.uk/catal/newsletter5/animal98.html:

> Large quantities of animal bones and teeth were recovered during the 1998 field season. . . . Sheep and goat bones predominate, while lower frequencies of cattle, wild horses and asses, red deer, wild boar, foxes, dogs and birds were found.

They provide additional information at

<http://catal.arch.cam.ac.uk/catal/newsletter3/other.html>:

> At least two species of equids (Equus hydruntinus and a large form, probably E. caballus) form a small but significant part of the assemblage.

And Sheelagh Frame with Nerissa Russell and Louise Martin add at

<http://catal.arch.cam.ac.uk/catal/archive%5Frep99/framemartin99.html>

that in addition to

> sheep, goat, cattle, wild horses, two species of wild ass, red deer, roe deer, wild boar, foxes, wolves, dogs and bear, we can now add fallow deer, wild cat, gazelle, badger and hedgehog, as well some small mustelid species. . . .

They additionally say,

> There are roughly equal proportions of cattle, equid, and sheep/goat bones. . . . The deposit contains chiefly meaty bones, with minimal processing (mostly broken for marrow). There is a certain repetition of body parts across taxa (radius of sheep and cattle, scapula of sheep and cattle, pelvis and distal tibia of cattle and equid). These are not meat offerings, because they were deposited in an already-processed state.

Similar evidence has apparently been obtained from the aceramic Neolithic site of Aşıklı Höyük in Cappadocia.[3] Equid remains from other Neolithic sites in Turkey have been sparse, as several reports in Meadow and Uerpmann (1986 and 1991) attest.

Çatalhöyük Wall Paintings

The presence of equids on the wall paintings from Çatalhöyük was recognized by Mellaart, and clear pictures (photographs and drawings) have been published. Yet there is this astonishing statement by Louise Martin and Nerissa Russell:

> The presence of moderate numbers of equids is particularly interesting given that they seem to be totally absent from the art, in contrast to the rarer deer. Horses were apparently not symbolically salient at Çatalhöyük. Perhaps speed was not important?

This is clearly inaccurate (and is an indication of the severe risks involved in the immediate and undigested publication of field reports through the WWW).

Equids are clearly and unmistakably depicted on a number of paintings. A selection from Mellaart 1966 suffices to make the point: Pls. LI (a), LIV (a & b) and LVII in a row below the bull, LII (a & b) bottom right, LIII (a & b) bottom center, LIV "wild asses", LXII-LXIII "onagers." The first and last of these examples form part of the excellent display in the Museum of Anatolian Civilizations at Ankara. The distribution of "wild asses" and "onagers" in the *Hunting Shrine* F.V.1 is shown on Mellaart's Fig. 10 (p. 187). Depictions in the "Hunting Shrine" apparently show some of the equids being hunted. Mellaart considered the possibility that horses were represented since, on page 188, he describes "seven wild asses (not horses)." On page 190 Mellaart vividly describes "a most attractive scene of two brown onagers with black outline along the back, a bushy tail and black-tipped ears." He went on to compare this pair of opposed equids, each gracefully looking back over its shoulder, with Paleolithic cave paintings in France, and, indeed, they are one of the most accomplished artistic representations of animals at Çatal.

It is perhaps pertinent to wonder whether Mellaart would have been so certain of his identifications had he been aware of the presence of horse bones amongst the faunal assemblage. It might also be asked whether or not it is possible to identify horses amongst the various depictions of equids on the Çatal wall paintings, something that I do not feel competent to do.

Summary of the Evidence for Equids in Neolithic Anatolia.

The osteological evidence from Çatal supports the view that three species of equid were present and were a source of food. One of these species was wild horse (probably *Equus caballus,* as noted above). Similar evidence apparently comes from Aşıklı Höyük, showing that the horse was not restricted to the grasslands of the Konya Plain but existed in more broken country well to the northeast.[4] The wall paintings clearly depict one or more species of equid, at least one of which, Mellaart's "wild asses," is being hunted.[5] Full publication of the osteological evidence and the ongoing research at Çatalhöyük, e.g., the possibility that micromorphological examination being conducted by Dr. Wendy Matthews might reveal recognizable traces of horse hair, hide, or dung, will shed more light on this emerging view.

Chalcolithic and Early Early Bronze Age Evidence for Horses in Anatolia

There was, however, one exceptional piece of evidence from the western end of the Anatolian Plateau, from the site of Demirci Hüyük near modern Eskişehir. From here, in a specialist volume published in 1987, Boessneck and von den Driesch reported that wild horses were common from the Neolithic to the Early Bronze Age. News of this discovery was reported by Korfmann as early as 1978, but the identification was generally disregarded, e.g., by Mellaart (1981: 137), who, with typical flamboyance, assumed that the horses were domesticated.

The presence of horse bones from the Chalcolithic site of Yarıkkaya, near Boğazköy (Mellaart 1981: 137)[6] in northern central Anatolia, is of interest. The precise date of Yarıkkaya is unclear, although it should predate the final phase of the Chalcolithic. There is no reason to assume that these horses were domesticated (but see note 3).[7] If they were wild horses, there are implications concerning both the extent of the forest and adaptation of horse behavior to a more wooded environment.[8]

Occasional reports of early horse bones from archaeological excavations in eastern Anatolia have rarely been discussed outside archaeozoological circles (e.g., Meadow and Uerpmann 1986 and 1991; Uerpmann 1987). Some of these reports were reviewed by Sándor Bökönyi (1983). Additional reports include Deniz (1975) on Pulur Höyük (Keban), who reported *Equus ferus*. The study of the osteological material from Norşuntepe, Tülintepe and Tepecik, sites in the Altınova now flooded by the Keban Dam, published by Boessneck and von den Driesch (1976)[9] suggested that the bones came from wild horses that were occasionally hunted. Bökönyi (1986 and 1991) identified horse bones from two other sites in the Euphrates region, Değirmentepe and Arslantepe. Bökönyi, however, believed that all of the horses in the upper Euphrates region were domesticated. Bökönyi seems to have continued to believe that the early horses were domesticated because in his later study of hunted animals at Arslantepe (Bökönyi 1993) the only equid in his list is *E. hemionus* (onager). The strongest argument that there were no wild horses in Neolithic Anatolia put forward by Bökönyi can now be dismissed. A further difficulty with his interpretation is the evidence now available that horse bones are absent from Early Bronze Age I levels at Arslantepe (Bartosiewicz 1998).[10] Uerpmann (1987: 13–19) accepts the view put forward by Boessneck and von den Driesch (1976) that domestic horses were introduced sometime in the Early Bronze Age.

There is one tantalizing piece of evidence for the Chalcolithic: *Excavations on the West Mound at Çatalhöyük 1998*, by Jonathan Last, which can be found at <http://catal.arch.cam.ac.uk/catal/archive_rep98/lastwest98.html>:

> Faunal remains from pit 2941 in Trench II include relatively high numbers of large equids, raising the question of possible horse domestication at an early date.

But more circumspect is *Çatalhöyük Animal Bone Report* by Nerissa Russell and Louise Martin at

> <http://catal.arch.cam.ac.uk/catal/archive%5Frep98/martin98.html>:
>
> Among the equid remains from Çatalhöyük West, several bones were observed as belonging to "large" equids, which is likely to mean *Equus caballus*. If these identifications are confirmed, this would greatly alter our knowledge of the distribution of wild horses in the Holocene.

Conclusions

There were populations of wild horses on the Anatolian Plateau in the Neolithic period. Some parts of this population appear to have survived into the Late Chalcolithic period in both the upper Euphrates region and in central Anatolia. These animals would seem to have been hunted. There is not yet any evidence for wild horse populations in the Early Bronze Age, thus we may assume that these animals had become very rare or were extinct in the early third millennium. There is no good evidence that the horse was first domesticated in Anatolia and, although the tentative suggestion of evidence from Çatalhöyük West has been noted above, this scenario might seem unlikely. On present evidence, scant though it is, domestic horses would seem to have been introduced into all parts of Anatolia in the Bronze Age.

Further finds of wild horse bones can be expected from the central Plateau and eastern Anatolia, but perhaps not from Turkish Thrace, the far west or the southeast of Turkey. The concentration of discoveries of horse bones from Chalcolithic sites in the Altınova is most probably a reflection of the concentration of archaeological research associated with the construction of the Keban Dam. The absence of horse bones from Neolithic sites in the same region simply reflects the paucity of excavated sites of that period.

The Neolithic and Chalcolithic peoples of central and eastern Anatolia would have names for the various equid species, including horses, depicted on the walls of Çatalhöyük and evidenced by the osteological remains.

Notes

I am grateful to Robert Drews for inviting me to contribute this short note, to Roger and Wendy Matthews and Norbert Karg for discussion on several points.

1. For the most recent and most elaborate exposition of the thesis, which makes no mention of horses, see Renfrew 1999.

2. One result of the redating is that we need no longer be in doubt over whether evi-

dence for particular forms of tooth wear can be taken to indicate the use of the bit.

3. Summarized in Esin 1998a; 1998b; Esin and Harmankaya 1999, which all report the presence of "wild horse," but Buitenhuis 1996 only gives *Equus* spec.

4. The central highlands of Anatolia, known from the time of the Persian Empire as Cappadocia, were famous for their horses (e.g., see Hyland: 9-10, 108-13).

5. Ian Todd, in his retrospective study (1976), makes no mention of horses but, following Mellaart, only of onagers and wild asses.

6. The original report by Boessneck and Wiedemann (1977: 121) is not available to me.

7. The Bronze Age attribution given by Uerpmann (1987: 15, table 1b, site no. 94) is too late.

8. Some of these matters are being studied by Schoop. For the time being, see Schoop 1998.

9. This study is not available to me.

10. This negative evidence is not, of course, proof; but the extent of the excavations and the quantities of bone material recovered lend much weight to the conclusions.

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